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THE TRANSACTIONS

OF THE

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

VOL. IV.—NEW SERIES.

SESSION 1884-85.

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PREFACE.

THE present Volume is the *Fourth* of the *New Series*, and contains a record of the work done during the past Session.

That work, as hitherto, embraces the communication of Original Papers; the exhibition of Patients, illustrating rare and interesting forms of disease; and the exhibition of Pathological and other specimens, so essential to the proper understanding of the morbid changes which take place in the human body.

During the past Session several Extra Meetings were held for Clinical and Pathological Demonstrations, and it is hoped that such Meetings will materially increase the usefulness of the Society.

It is believed that the publication of the Transactions in this permanent form will prove a valuable contribution to medical literature, will encourage the Members to take a more active part in the work of the Society, and will tend in no small degree to increase the influence and usefulness of the Medico-Chirurgical Society of Edinburgh.

WILLIAM CRAIG,
Editor.

October 1885.

Medico-Chirurgical Society of Edinburgh.

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	* Thomas Rutherford Ronaldson, M.B., F.R.C.P. Ed., 18 Bruntsfield Place,	1877
	George D. Smith, M.D., M.R.C.P. Ed., The Priory, Newhaven Road,	1877
115	James Stitt Thomson, M.R.C.P. Ed., Dalkeith,	1877
	Bryan Charles Waller, M.B., F.R.C.S. Ed., Masongill House, Carnforth,	1877
	* John Graham Brown, M.D., F.R.C.P. Ed., 16 Ainslie Place,	1878
	Alexander Robert Coldstream, M.B., F.R.C.S. Ed., Florence, Italy,	1878
	* Joseph Montagu Cotterill, M.B., F.R.C.S. Ed., 23 Walker Street,	1878
120	George Mackay, M.B., F.R.C.S. Ed., 2A Gilmore Place,	1878
	David Menzies, M.B., F.R.C.S. Ed., 21 Rutland Square,	1878
	James Allan Philip, M.B., Monte Carlo, Italy,	1878
	John Shand, M.D., F.R.C.P. Ed., 34 Albany Street,	1878
	* Johnson Symington, M.D., F.R.C.S. Ed., 10 Warrender Park Crescent,	1878
125	William Barrie Dow, M.D., F.R.C.S. Ed., Dunfermline,	1879
	John Fraser, M.B., M.R.C.P. Ed., 19 Strathearn Road,	1879
	Richard Freeland, M.B., C.M., Broxburn,	1879
	James Allan Gray, M.D., F.R.C.P. Ed., 107 Ferry Road,	1879
	* Peter M'Bride, M.D., F.R.C.P. Ed., 16 Chester Street,	1879
130	William Stewart, M.B., C.M., Kirkwall,	1879
	Charles H. Fasson, Dep. Surgeon-General, Royal Infirmary,	1879
	A. D. Leith Napier, M.D., C.M., Dunbar,	1879
	Andrew Fleming, M.D., Dep. Surgeon-General, 8 Napier Road,	1880
	Thomas Duddingston Wilson, M.B., F.R.C.S. Ed., 5 West Newington Terrace,	1880
135	T. Fair H. Spence, M.B., C.M., 8 Castle Terrace,	1881
	George Leslie, M.B., C.M., Old Manse, Falkirk,	1881
	Robert Lawson, M.D., C.M., 24 Mayfield Terrace,	1881
	John Hutton Balfour, M.B., C.M., Portobello,	1881
	Alexander Hugh Freeland Barbour, M.D., F.R.C.P. Ed., 24 Melville Street,	1881
140	George Alexander Gibson, M.D., F.R.C.P. Ed., 17 Alva Street,	1881
	William Badger, M.B., C.M., Penicuik,	1882
	Alexander Fergusson, M.D., F.R.C.S. Ed., Peebles,	1882
	Alexander Matthew, F.R.C.S. Ed., Corstorphine,	1882
	John Archibald, M.B., F.R.C.S. Ed., Lynton House, Brixton Rise, Brixton, London, S.W.,	1882
145	James Maxwell Ross, M.B., F.R.C.S. Ed., 112 Gilmore Place,	1882
	John Carlyle Johnstone, M.B., C.M., Morningside Asylum,	1882
	James Rutherford Morison, M.B., F.R.C.S. Ed., Hartlepool,	1882
	Roderick Maclaren, M.D., 23 Portland Square, Carlisle,	1882
	W. Wotherspoon Ireland, M.D., Prestonpans,	1883
150	Francis Mitchell Caird, M.B., F.R.C.S. Ed., 8 Torphichen St.	1883
	F. W. Dyce Fraser, M.D., F.R.C.P. Ed., 20 Chester Street,	1883
	James Longmuir, M.D., Bathgate,	1883
	Robert Henry Blaikie, M.B., C.M., 2 Newington Terrace,	1883

		Date of Admission.
	R. M'Kenzie Johnstone, M.B., C.M., 5 Rutland Square,	1883
155	Charles Walker Cathcart, M.B., F.R.C.S. Ed., 44 Melville Street,	1883
	Alexander Bruce, M.B., F.R.C.P. Ed., 13 Alva Street,	1883
	Andrew Semple, M.D., F.R.C.S. Ed., Dep. Surgeon-General, 8 Abercromby Place,	1883
	William Hy. Shirreff, M.B., C.M., 29 Castle Terrace,	1883
	John Lyon Wilson, L.R.C.P. Ed., 44 South Clerk Street,	1883
160	Donald MacRaild, F.R.C.S. Ed., Greenock,	1883
	Henry Newcombe, M.D., F.R.C.S. Ed., 5 Dalrymple Crescent,	1883
	Francis Troup, M.D., M.R.C.P. Ed., 1 Minto Street,	1883
	Russell Eliott Wood, M.B., C.M., 9 Darnaway Street,	1883
	John Macdonald Brown, M.B., F.R.C.S. Ed., 6 Atholl Place,	1883
165	James William Beeman Hodsdon, M.D., M.R.C.P. Ed., 30 Walker Street,	1883
	John Haddon, M.D., C.M., Honeyburn, Hawick,	1883
	Germon Sims Woodhead, M.D., F.R.C.P. Ed., 6 Marchhall Crescent,	1883
	Thomas Francis Spittal Caverhill, M.B., F.R.C.P. Ed., 8A Abercromby Place,	1883
	Robert Alexander Lundie, M.B., B.Sc., 35 Warrender Park Road,	1883
170	Arthur W. Hare, M.B., C.M., 21 Ainslie Place,	1883
	Edwin Baily, M.B., C.M., Oban,	1883
	Alexander Black, M.B., F.R.C.P. Ed., 8 St Vincent Street,	1883
	Harry Melville Dunlop, M.B., C.M., Middlefield House, Leith Walk,	1883
	George Andreas Berry, M.B., F.R.C.S. Ed., 23 Rutland Street,	1883
175	Hamilton Wylie, M.B., C.M., 1 George Place,	1883
	James Bennet, M.B., C.M., 49 Charlotte Street, Leith,	1883
	Arthur Douglas Webster, M.D., M.R.C.P. Ed., 10 West Newington Terrace,	1883
	Robert William Philip, M.B., C.M., 12 Hope Street,	1883
	William M'Neil, M.D., Stranraer,	1883
180	Joseph Carne Ross, M.D., F.R.C.P. Ed., Penzance,	1884
	William Russell, M.B., M.R.C.P. Ed., 33 Gayfield Square,	1884
	George Dickson, M.D., F.R.C.S. Ed., 9 India Street,	1884
	Thomas Wyld Pairman, L.R.C.P. & S. Ed., Stogumber, Taunton,	1884
	Alexander Thom, jr., M.D., C.M., Crieff,	1884
185	Hugh Logan Calder, M.D., C.M., 42 Leith Walk,	1884
	James Craig Balfour, L.R.C.P. & S. Ed., 25 Minto Street,	1884
	Frederick Anastasius Saunders, L.R.C.P. & S. Ed., Denburn, Crail,	1884
	William Richardson, F.R.C.S. Ed., Bath Lodge, Reading,	1884
	Andrew Brown, M.D., M.R.C.P. Ed., 1 Bartholomew Road, Kentish Town, London, N.W.	1884
190	G. J. H. Bell, M.B., C.M., Barrow-in-Furness,	1884
	T. Goodall Nasmyth, M.B., C.M., Cowdenbeath, Fife,	1884
	Henry Hay, M.B., C.M., 10 Brandon Street,	1884
	Thomas R. Scott, M.B., C.M., Musselburgh,	1884
	R. Milne Murray, M.B., M.R.C.P. Ed., 10 Hope Street,	1884
195	A. Murray Gibson, M.D., Portobello,	1884
	A. S. Cumming, M.B., C.M., 18 Ainslie Place,	1884
	Ernest F. Neve, M.D., C.M., The Cowgate Dispensary,	1884
	W. C. Greig, M.B., C.M., 13 Arniston Place,	1884
	William Wilson, M.B., C.M., 21 Young Street,	1885
200	John Mowat, M.D., 1 Hope Park Terrace,	1885
	Skene Keith, M.B., F.R.C.S. Ed., 2 North Charlotte Street,	1885
	D. Noel Paton, M.D., C.M., 21 Maitland Street,	1885
	George Hugh Mackay, M.B., C.M., Elgin,	1885
	J. Michael Dewar, M.B., C.M., 110 Lauriston Place,	1885
205	Edward M'Callum, F.R.C.S. Ed., 3 Brandon Street,	1885

		Date of Admission.
	T. Edgar Underhill, M.B., F.R.C.S. Ed., Tipton Green, Staffordshire,	1885
	John Struthers Stewart, L.R.C.P. & S. Ed., 16 Merchiston Terrace,	1885
	Allen Thomson Sloan, M.D., C.M., 22 Forth Street,	1885
	John William Ballantyne, M.B., C.M., 50 Queen Street,	1885
210	James Robertson Crease, F.R.C.S. Ed., 2 Ogle Terrace, South Shields,	1885
	George Kerr, M.B., C.M., 9 Great Stuart Street,	1885
	Tom Bairstow, L.R.C.P. & S. Ed., 13 Buccleuch Place,	1885

NON-RESIDENT.

	Arthur Edward Turnour, M.D., M.R.C.S. Eng., *Denbigh*,	1843
	George Skene Keith, M.D., F.R.C.P. Ed., *Currie*,	1845
215	W. Judson Van Someren, M.D., L.R.C.S. Ed., *Redhill, Surrey*,	1845
	W. Ord M'Kenzie, M.D., L.R.C.S. Ed., *London*,	1845
	W. Chalmers Fowler, M.D., M.R.C.S. Eng., *London*,	1847
	Veitch Sinclair, L.R.C.P. and S. Ed., *London*,	1850
	Andrew Graham, M.D., Fleet Surgeon, R.N.,	1853
220	Archibald Hall, M.D., *Montreal*,	1853
	John Traill, F.R.C.S. Ed., *Arbroath*,	1853
	William H. Lowe, M.D., F.R.C.P. Ed., *Wimbledon*,	1854
	W. Overend Priestly, M.D., LL.D., F.R.C.P. Ed., *London*,	1854
	Horatio Robinson Storer, M.D., *Newport, Rhode Island, U.S.*,	1855
225	James C. Howden, M.D., *Montrose*,	1856
	Thomas Skinner, M.D., L.R.C.S. Ed., *London*,	1856
	J. Ivor Murray, M.D., F.R.C.S. Ed., *Scarboro'*,	1857
	Professor William Smoult Playfair, M.D., F.R.C.P.L., *London*,	1857
	Andrew Scott Myrtle, M.D., L.R.C.S. Ed., *Harrogate*,	1858
230	Robert Foulis, M.D., F.R.C.S. Ed., *Cupar-Fife*,	1859
	Francis Robertson Macdonald, M.D., *Inveraray*,	1859
	Professor John Young, M.D., *University of Glasgow*,	1859
	J. Cecil Phillippo, M.D., *Kingston, Jamaica*,	1860
	Norman Bethune, M.D., F.R.C.S. Ed., *Toronto*,	1861
235	T. Spencer Cobbold, M.D., F.R.S., *London*,	1861
	Peter Gordon, L.R.C.P. and S. Ed., *Juniper Green*,	1861
	Professor William Stephenson, M.D., F.R.C.S. Ed., *Aberdeen*,	1861
	George Thin, M.D., L.R.C.S. Ed., *London*,	1861
	David Yellowlees, M.D., F.F.P.S. Glasg., *Glasgow*,	1862
240	Prof. Arthur Gamgee, M.D., F.R.C.P. Ed., F.R.S., *Manchester*,	1863
	William M'Cullock Watson, M.D., *Montrose*,	1863
	Professor John Cleland, M.D., LL.D., *The University, Glasgow*,	1864
	R. B. Finlay, M.D., *Middle Temple, London*,	1864
	Stanley Lewis Haynes, M.D., M.R.C.S. Eng., *Malvern*,	1864
245	Francis D. A. Skae, M.D., *Lerwick*,	1864
	James Watt Black, M.D., F.R.C.P.L., *London*,	1865
	David Brodie, M.D., *Canterbury*,	1865
	Thomas Sheriff, L.R.C.P. and S. Ed., *Edinburgh*,	1867
	Lauchlan Aitken, M.D., C.M., *Rome*,	1868
250	Peter Maury Deas, M.B., L.R.C.S. Ed., *Exeter*,	1868
	J. G. Sinclair Coghill, M.D., F.R.C.P. Ed., *Ventnor*,	1870
	Professor J. G. M'Kendrick, M.D., F.R.C.P. Ed., *University, Glasgow*,	1870
	Lawson Tait, F.R.C.S. Ed. and Eng., *Birmingham*,	1870
	J. William Eastwood, M.D., M.R.C.P.L., *Darlington*,	1871
255	James Johnston, M.D., L.R.C.S. Ed., *Shanghai*,	1871
	Professor J. Bell Pettigrew, M.D., F.R.C.P. Ed., *University of St Andrews*,	1873

		Date of Admission.
	John Smith,	1873
	Frederick William Barry, M.D., D.Sc., *London*,	1874
	Dr Groesbeck, *Cincinnati*,	1875
260	John Aymers Macdougall, M.D., F.R.C.S. Ed., *Carlisle*,	1875
	Thomas John Maclagan, M.D., M.R.C.P.L., *London*,	1875
	Professor F. S. B. de Chaumont, M.D., F.R.C.S. Ed., *Netley*,	1876
	Professor David James Hamilton, M.B., F.R.C.S. Ed., *Aberdeen University*,	1876
	J. Moolman, M.B., C.M., *Cape of Good Hope*,	1877
265	Robert Somerville, M.D., L.R.C.S. Ed., *Galashiels*,	1877
	Graham Steell, M.D., M.R.C.P.L., *Manchester*,	1877
	John Brown, M.D., F.R.C.S. Eng., *Burnley*,	1878
	Thomas Inglis, F.R.C.P. Ed., *Lincoln*,	1878
	Walter Weir, M.B., F.R.C.P. Ed., *London*,	1879
270	Keith Norman Macdonald, M.D., F.R.C.P. Ed., *Cupar-Fife*,	1880
	John Home Hay, M.D., M.R.C.S. Eng., *Alloa*,	1880
	John Mackay, M.D., L.R.C.S. Ed., *Aberfeldy*,	1881

ORDINARY MEMBERS,

ARRANGED ALPHABETICALLY.

RESIDENT.

	Dr J. O. Affleck, 38 Heriot Row,	1871
	Dr James Andrew, 2 Atholl Crescent,	1869
	Professor Annandale, 34 Charlotte Square,	1863
	Dr Archibald, Lynton House, Brixton Rise, Brixton, London,	1882
5	Dr W. Badger, Penicuik,	1882
	Dr J. Johnson Bailey, Marple, Cheshire,	1874
	Dr Edwin Baily, Oban,	1883
	Tom Bairstow, Esq., 13 Buccleuch Place,	1885
	Dr Andrew Balfour, Portobello,	1874
10	Dr J. H. Balfour, Portobello,	1881
	Dr G. W. Balfour, 17 Walker Street,	1874
	Dr James Craig Balfour, 25 Minto Street,	1884
	Dr Thomas Balfour, 51 George Square,	1856
	Dr Alexander Ballantyne, Dalkeith,	1873
15	Dr J. W. Ballantyne, 50 Queen Street,	1885
	Dr A. H. Freeland Barbour, 24 Melville Street,	1881
	Dr G. J. H. Bell, Barrow-in-Furness,	1884
	Joseph Bell, Esq., 2 Melville Crescent,	1862
	Dr James Bennet, 49 Charlotte Street, Leith,	1883
20	G. H. Bentley, Esq., Kirkliston,	1877
	Dr G. A. Berry, 23 Rutland Street,	1883
	Dr James S. Beveridge, 8 Eildon Street,	1861
	Dr Bishop,	1872
	Dr Alexander Black, 8 St Vincent Street,	1883
25	Dr W. T. Black, 2 George Square,	1877
	Dr Robert H. Blaikie, 2 Newington Terrace,	1883
	Dr Bleloch, 2 Lonsdale Terrace,	1871
	Dr Brakenridge, 10 St Colme Street,	1865
	Dr Byrom Bramwell, 23 Drumsheugh Gardens,	1876
30	Dr Brown, 1 Bartholomew Road, Kentish Town, London,	1884
	Dr J. Graham Brown, 16 Ainslie Place,	1878
	Dr J. Macdonald Brown, 6 Atholl Place,	1883
	William Brown, Esq., 25 Dublin Street,	1821
	Dr Alexander Bruce, 13 Alva Street,	1883
35	Dr Robert Bruce, 12 York Place,	1858
	Dr Buist, 1 Clifton Terrace,	1877

		Date of Admission.
	Dr Burn, 45 Lauriston Place,	1847
	Dr Cadell, 5 Castle Terrace,	1870
	Dr Francis M. Caird, 8 Torphichen Street,	1883
40	Dr H. L. Calder, 42 Leith Walk,	1884
	Dr W. Watson Campbell, Duns,	1877
	Dr Cappie, 47 Lauriston Place,	1855
	Dr J. Carmichael, 22 Northumberland Street,	1870
	Dr C. W. Cathcart, 44 Melville Street,	1883
45	Dr T. F. S. Caverhill, 8A Abercromby Place,	1884
	Professor John Chiene, 26 Charlotte Square,	1867
	Dr Church, 36 George Square,	1876
	Dr Clouston, Tipperlinn House, Morningside Place,	1861
	Dr A. R. Coldstream, Florence,	1878
50	Dr John Connel, Peebles,	1876
	Dr Cotterill, 23 Walker Street,	1878
	Dr William Craig, 7 Bruntsfield Place,	1869
	Dr J. R. Crease, 2 Ogle Terrace, South Shields,	1885
	Dr Halliday Croom, 25 Charlotte Square,	1870
55	Dr A. S. Cumming, 18 Ainslie Place,	1884
	Dr William Cumming, 18 Ainslie Place,	1845
	Dr R. J. B. Cunynghame, 6 Walker Street,	1868
	Dr J. M. Dewar, 110 Lauriston Place,	1885
	Dr Archibald Dickson, 11 Royal Circus,	1871
60	Dr George Dickson, 9 India Street,	1884
	Dr Halliday Douglas, 30 Melville Street,	1842
	Dr William B. Dow, Dunfermline,	1879
	Dr John Duncan, 8 Ainslie Place, *Vice-President*,	1865
	Dr Kirk Duncanson, 22 Drumsheugh Gardens,	1870
65	Dr H. M. Dunlop, Middlefield House, Leith Walk,	1883
	Dr Dunsmure, 53 Queen Street,	1841
	Dr J. Dunsmure, jun., 53 Queen Street,	1872
	C. H. Fasson, Esq., Dep. Surg.-Gen., Royal Infirmary,	1879
	Dr Alexander Fergusson, Peebles,	1882
70	Dr Finlay, St Colms, Lennox Row,	1847
	Dr W. A. Finlay, St Helens, Russell Place,	1874
	Dr Andrew Fleming, 8 Napier Road,	1880
	Dr Foulis, 34 Heriot Row,	1875
	Dr F. W. Dyce Fraser, 20 Chester Street,	1883
75	Dr John Fraser, 19 Strathearn Road,	1879
	Professor Thomas R. Fraser, 37 Melville Street,	1865
	Dr R. Freeland, Broxburn,	1879
	Dr Garland, 35 Charlotte Street, Leith,	1873
	Dr A. Murray Gibson, Portobello,	1884
80	Dr G. A. Gibson, 17 Alva Street,	1881
	Dr James D. Gillespie, 10 Walker Street,	1850
	G. R. Gilruth, Esq., 67 York Place,	1869
	Dr J. Allan Gray, 107 Ferry Road,	1879
	Dr David Greig, 38 Coates Gardens,	1854
85	Dr W. C. Greig, 13 Arniston Place,	1884
	Dr R. H. Gunning, 30 Hazlitt Road, West Kensington, London,	1854
	Dr John Haddon, Honeyburn, Hawick,	1883
	Dr Haldane, 22 Charlotte Square,	1853
	Dr A. W. Hare, 21 Ainslie Place,	1883
90	Dr Henry Hay, 10 Brandon Street,	1884
	Dr John Henderson, 7 John's Place, Leith	1848
	Dr J. W. B. Hodsdon, 30 Walker Street,	1883
	Dr George Hunter, Linlithgow,	1877
	Dr James Hunter, 18 Abercromby Place,	1850
95	Dr Husband, 28 Clarence Street,	1850
	Francis B. Imlach, Esq., 48 Queen Street,	1843
	Dr Archibald Inglis, 33 Albany Street,	1827
	Dr W. Wotherspoon Ireland, Prestonpans	1883

		Date of Admission.
	Dr James, 11 Albyn Place, *Secretary*,	1877
100	Dr W. Allan Jamieson, 26 Rutland Street,	1876
	Dr James Jamieson, 43 George Square,	1877
	Dr R. M'Kenzie Johnston, 5 Rutland Square,	1883
	Dr J. Carlyle Johnstone, Morningside Asylum,	1882
	Dr Kalley, 9 Tipperlinn Road,	1860
105	Dr Keiller, 21 Queen Street,	1845
	Dr Skene Keith, 2 North Charlotte Street,	1885
	Dr Thomas Keith, 2 North Charlotte Street,	1852
	Dr George Kerr, 9 Great Stuart Street,	1885
	Dr Robert Lawson, 24 Mayfield Terrace,	1881
110	Dr George Leslie, Falkirk,	1881
	Dr Linton, 60 George Square,	1863
	Dr Littlejohn, 24 Royal Circus, *President*,	1853
	Dr James Longmuir, Bathgate,	1883
	Dr Lucas, Dalkeith,	1875
115	Dr R. A. Lundie, 35 Warrender Park Road,	1883
	Dr P. M'Bride, 16 Chester Street,	1879
	Dr E. M'Callum, 3 Brandon Street,	1885
	Dr Angus Macdonald, 29 Charlotte Square,	1866
	John M'Gibbon, Esq., 55 Queen Street,	1868
120	Dr MacGillivray, 11 Rutland Street, *Secretary*,	1877
	Dr G. Mackay, 2A Gilmore Place,	1878
	Dr G. H. Mackay, Elgin,	1885
	Professor Maclagan, 28 Heriot Row,	1834
	Dr P. H. Maclaren, 1 Drumsheugh Gardens,	1868
125	Dr Roderick M'Laren, 23 Portland Square, Carlisle,	1882
	Dr William M'Neil, Stranraer,	1883
	Dr Donald MacRaild, Greenock,	1883
	Dr A. Matthew, Corstorphine,	1882
	Dr D. Menzies, 21 Rutland Square,	1878
130	Dr W. Menzies, 3 Lothian Road,	1847
	A. G. Miller, Esq., 11 Walker Street, *Treasurer*,	1866
	Dr Arthur Mitchell, 34 Drummond Place,	1859
	Dr Moinet, 2 Abercromby Place,	1870
	Dr Moir, 52 Castle Street,	1836
135	Dr Alexander Moir, 30 Buccleuch Place,	1876
	Dr J. Rutherford Morison, Hartlepool,	1882
	Dr John Mowat, 1 Hope Park Terrace,	1885
	Dr Claud Muirhead, 30 Charlotte Square,	1866
	Dr R. Milne Murray, 10 Hope Street,	1884
140	Dr A. D. Leith Napier, Dunbar,	1879
	Dr T. Goodall Nasmyth, Cowdenbeath, Fife,	1884
	Dr E. F. Neve, Cowgate Dispensary,	1884
	Dr H. Newcombe, 5 Dalrymple Crescent,	1883
	Dr P. Orphoot, 113 George Street,	1865
145	Dr T. W. Pairman, Stogumber, Taunton,	1884
	Dr Paterson, 15 Merchiston Park,	1837
	Dr D. Noel Paton, 21 Maitland Street,	1885
	Dr Peddie, 15 Rutland Street,	1842
	Dr J. A. Philip, Italy,	1878
150	Dr R. W. Philip, 12 Hope Street,	1883
	Dr Playfair, 25 Rutland Street,	1874
	J. D. Pridie, Esq., 6 Minto Street,	1857
	Dr Rattray, Portobello,	1874
	Dr William Richardson, Bath Lodge, Reading,	1884
155	Dr James Ritchie, 14 Charlotte Square,	1873
	Dr R. Peel Ritchie, 1 Melville Crescent,	1861
	Dr Argyll Robertson, 18 Charlotte Square,	1861
	Dr Ronaldson, 18 Bruntsfield Place,	1877
	Dr J. Maxwell Ross, 112 Gilmore Place,	1882
160	Dr Joseph C. Ross, Penzance,	1884

		Date of Admission.
	Dr William Russell, 33 Gayfield Square,	1884
	Professor Rutherford, 14 Douglas Crescent,	1874
	Dr F. A. Saunders, Denburn, Crail,	1884
	Dr Thomas R. Scott, Musselburgh,	1884
165	Dr Andrew Semple, 8 Abercrombie Place,	1883
	Dr John Shand, 34 Albany Street,	1878
	C. H. E. Sheaf, Esq., Barrow-in-Furness,	1871
	Dr W. H. Shirreff, 29 Castle Terrace,	1883
	Dr J. Sibbald, 3 St Margaret's Road,	1858
170	Dr James Sidey, 20 Heriot Row,	1849
	Professor Simpson, 52 Queen Street,	1857
	Dr A. J. Sinclair, 21 Northumberland Street,	1873
	Dr A. T. Sloan, 22 Forth Street,	1885
	Dr Andrew Smart, 20 Charlotte Square,	1864
175	Dr G. D. Smith, The Priory, Newhaven Road,	1877
	Dr John Smith, 11 Wemyss Place,	1856
	Dr T. F. H. Spence, 8 Castle Terrace,	1881
	Professor Grainger Stewart, 19 Charlotte Square,	1860
	Dr J. S. Stewart, 16 Merchiston Terrace,	1885
180	Dr W. Stewart, Kirkwall,	1879
	Dr John Strachan, Dollar,	1867
	Dr James Struthers, 39 Charlotte Street, Leith,	1849
	Dr Johnson Symington, 10 Warrender Park Crescent,	1878
	Dr W. Taylor, 12 Melville Street,	1870
185	Dr C. H. Thatcher, 13 Albany Street,	1876
	Dr Alexander Thom, jr., Crieff,	1884
	Dr Alexander Thomson, 14 Rankeillor Street,	1849
	Dr J. Stitt Thomson, Dalkeith,	1877
	Dr Francis Troup, 1 Minto Street,	1883
190	Dr Batty Tuke, 20 Charlotte Square, *Vice-President*,	1863
	Professor Turner, 6 Eton Terrace,	1858
	Dr R. S. Turner, Keith,	1867
	Dr Underhill, 8 Coates Crescent,	1872
	Dr T. Edgar Underhill, Tipton Green, Staffordshire,	1885
195	B. C. Waller, Esq., M.B., Carnforth,	1877
	Dr P. H. Watson, 16 Charlotte Square,	1856
	Dr W. Watson, Midcalder,	1862
	Dr A. D. Webster, 10 West Newington Terrace,	1883
	Dr Graham Weir, 36 Heriot Row,	1837
200	Dr Williamson, 53 Charlotte Street, Leith,	1849
	Dr David Wilson, 12 Dean Terrace, *Vice-President*,	1844
	J. L. Wilson, Esq., 44 South Clerk Street,	1883
	Dr T. D. Wilson, 5 West Newington Terrace,	1880
	Dr William Wilson, 21 Young Street,	1885
205	Dr Russell E. Wood, 9 Darnaway Street,	1883
	Dr G. Sims Woodhead, 6 Marchhall Crescent,	1883
	Dr Strethill Wright, Southport,	1871
	Dr Hamilton Wylie, 1 George Place,	1883
	Dr John Wyllie, 1 Melville Street,	1868
210	Dr James Young, 14 Ainslie Place,	1858
	Dr P. A. Young, 25 Manor Place,	1870
	Dr Ziegler, 47 George Square,	1876

NON-RESIDENT.

	Dr Lauchlan Aitken, *Rome*,	1868
	Dr F. W. Barry, *London*,	1874
215	Dr Bethune, *Toronto*,	1861
	Dr J. W. Black, *London*,	1865
	Dr Brodie, *Canterbury*,	1865
	Dr John Brown, *Burnley*,	1878

		Date of Admission.
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220	Professor Cleland, *Glasgow*,	1864
	Dr Cobbold, *London*,	1861
	Dr Coghill, *Ventnor*,	1870
	Dr P. M. Deas, *Exeter*,	1868
	Dr J. W. Eastwood, *Darlington*,	1871
225	Dr R. B. Finlay, *Middle Temple, London*,	1864
	Dr Foulis, *Cupar-Fife*,	1859
	Dr Fowler, *London*,	1847
	Professor Gamgee, *Manchester*,	1863
	Peter Gordon, Esq., *Juniper Green*,	1861
230	Dr A. Graham, R.N.,	1853
	Dr Groesbeck, *Cincinnati*,	1875
	Dr Archibald Hall, *Montreal*,	1853
	Professor D. J. Hamilton, *Aberdeen University*,	1876
	Dr J. H. Hay, *Alloa*,	1880
235	Dr Stanley Haynes, *Malvern*,	1864
	Dr J. S. Howden, *Montrose*,	1856
	Dr T. Inglis, *Lincoln*,	1878
	Dr James Johnston, *Shanghai*,	1871
	Dr George Keith, *Currie*,	1845
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	Dr F. R. Macdonald, *Inveraray*,	1859
	Dr K. N. Macdonald, *Cupar-Fife*,	1880
	Dr John A. Macdougall, *Carlisle*,	1875
	Dr John Mackay, *Aberfeldy*,	1881
245	Professor M'Kendrick, *Glasgow*,	1870
	Dr W. O. Mackenzie, D.I.G.H., *London*,	1845
	Dr T. J. Maclagan, *London*,	1875
	Dr J. Moolman, *Cape of Good Hope*,	1877
	Dr J. Ivor Murray, *Scarboro'*,	1857
250	Dr Andrew Myrtle, *Harrogate*,	1858
	Professor Bell Pettigrew, *St Andrews*,	1873
	Dr Phillippo, *Kingston, Jamaica*,	1860
	Professor W. S. Playfair, *London*,	1857
	Dr Priestley, *London*,	1854
255	Thomas Sheriff, Esq., *Edinburgh*,	1867
	Dr Sinclair, *London*,	1850
	Dr Francis Skae, *Lerwick*,	1864
	Dr T. Skinner, *London*,	1856
	Dr John Smith,	1873
260	Dr Van Someren, *Redhill, Surrey*,	1845
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	Professor Stephenson, *Aberdeen*,	1861
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265	Lawson Tait, Esq., *Birmingham*,	1870
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TRANSACTIONS

OF

THE MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH,

FOR SESSION LXIV., 1884-85.

Meeting I.—November 5, 1884.

Dr H. D. Littlejohn, *President, in the Chair.*

I. Election of Office-Bearers.

The following gentlemen were appointed to act as office-bearers during the ensuing session:—*President,* Henry D. Littlejohn, M.D., F.R.C.S. Ed.; *Vice-Presidents,* David Wilson, M.D., F.R.C.S. Ed.; J. Batty Tuke, M.D., F.R.C.P. Ed.; John Duncan, M.A., F.R.C.S. Ed.; *Treasurer,* Alexander G. Miller, F.R.C.S. Ed., 11 Walker Street; *Secretaries,* C. Watson MacGillivray, M.D., F.R.C.S. Ed., 11 Rutland Street; Alexander James, M.D., F.R.C.P. Ed., 11 Albyn Place; *Editor of Transactions,* William Craig, M.D., F.R.C.S. Ed.; *Members of Council,* George Hunter, M.D., F.R.C.S. Ed., Linlithgow; James Jamieson, M.D., F.R.C.S. Ed.; J. Graham Brown, M.D., F.R.C.P. Ed.; J. M. Cotterill, M.B., F.R.C.S. Ed.; A. M. T. Rattray, M.D.; A. Moir, L.R.C.P. and S. Ed.; Johnson Symington, M.B., F.R.C.S. Ed.; P. M'Bride, M.D., F.R.C.P. Ed.

II. Election of New Members.

The following gentlemen were elected Ordinary Members of the Society:—William Richardson, F.R.C.S. Ed., Reading, Berks.; Andrew Brown, M.D., M.R.C.P. & L.R.C.S. Ed., Kentish Town, London, N.W.; G. J. H. Bell, M.B., C.M. Ed., Barrow-in-Furness, Lancashire; T. Goodall Nasmyth, M.B., C.M., B.Sc. Ed., Cowdenbeath, Fifeshire; Henry Hay, M.B., C.M. Ed., Edinburgh.

III. Exhibition of Instruments.

1. *Dr M'Bride* showed a set of ELECTRIC ACCUMULATORS, useful for electric light (for examination of nose and larynx), galvano-caustic instruments, electrolysis, and supplying motor power (*e.g.*, for driving a drill). Demonstrations were given illustrating each of these points, with the exception of electrolysis, which could not be shown to a large audience.

2. *Mr A. G. Miller* showed a SAW for dividing long bones *in situ*, the invention of Dr John Wyeth of New York. The instrument consisted of two parts—first, a claw by means of which the bone was grasped; and second, the saw, made like a chisel, but with a rounded edge. A very small incision was required to admit the claw. When it was fixed, the saw was run along a groove to the bone. He had tried it on the living subject, and found it work very sweetly and smoothly.

IV. Exhibition of Pathological Specimens.

1. *Dr Byrom Bramwell* showed TWO LUNGS containing patches of pulmonary apoplexy, and a portion of the RIGHT AURICLE of the HEART containing ante-mortem blood-clots; the branches of the pulmonary artery were plugged with very distinct infarctions, which were without doubt derived from the heart. Dr Bramwell had met with seven other cases during the past year in which the pulmonary arteries were plugged with infarctions, and in which there were patches of pulmonary apoplexy.

2. *Dr Allan Gray* showed (*a*) THE LARYNX AND TRACHEA FROM A CASE OF CROUP IN WHICH TRACHEOTOMY WAS PERFORMED. The patient was a child, æt. 15 months. Tracheotomy had to be performed, because of the suffocation which threatened. The croupous membrane was present above the site of the tracheal incision, but was gone below. On several occasions the tube was blocked, and once, it was almost thought, hopelessly, but detachment of the membrane was accomplished by means of a feather. The child died, two days and a half after the operation, of œdematous congestion of the lungs. (*b*) AORTA AND TRACHEA FROM A CASE IN WHICH A SMALL AORTIC ANEURISM RUPTURED INTO THE TRACHEA. The patient was a man who had been admitted to Leith Hospital, breathless and in great discomfort. The history was, that he was subject to chronic bronchitis and, for several weeks before admission, to attacks of breathlessness, from which he had recovered on each occasion without any apparent ill effects. The attack, for which he was admitted, had come upon him just as he was leaving a steamer in the docks. On admission, his case appeared to be one of pulmonary congestion; but examination of his chest, after he had been got quietly to bed,

failed to show anything beyond the ordinary signs of a chronic bronchitis. The *râles* heard were sufficient to prevent any exact stethoscopic examination of the heart. There was a feeble pulse, but beyond that little could be made out regarding the condition of the cardiac organ. In a few hours the attack passed off. The heart and bloodvessels were again examined, but no physical signs of disease observed. A second attack occurred a few days later. From this he also recovered, but a third attack was fatal. He coughed up a large quantity of blood; and was dead. It was quite evident then, as the autopsy afterwards showed, that an aneurism had ruptured into the windpipe. The opening into the trachea was well seen.

3. *Mr Johnson Symington* showed a specimen of ACQUIRED FLAT-FOOT. This specimen was of interest on account of the fact that although the deformity is a very common one there are no specimens in any of the museums in this country. In this case there was a general relaxation of the ligaments, and contrary to the statement of Professor Hermann von Meyer, the inferior calcaneo-scaphoid ligament was increased in length. The most marked displacement occurred at the articulation between the astragalus and the rest of the tarsus. Those interested in the subject were referred to a paper by Mr Symington in the October number of the *Journal of Anatomy and Physiology.*

4. *Dr Argyll Robertson* showed an eye removed for a MELANOTIC SARCOMA AFFECTING THE CONJUNCTIVA OF THE LID AND GLOBE. The eye itself was not affected. There was perfect vision, and ophthalmoscopic examination failed to detect any lesion in the interior of the globe. The history was one of very rapid growth. One month before admission, the patient, a woman, felt a small lump or tumour on the upper lid, close to the oculo-palpebral conjunctival fold. She did not pay much attention till a hæmorrhage from the lid made her consult her doctor, who advised her to come to the Edinburgh Infirmary. On admission, part of the conjunctiva over the globe, as well as over the lid, was found to be affected. During the three days before operation the tumour almost doubled in size. The eye-ball was removed in order to eradicate the whole disease, and because it would have been left without suitable covering had the affected parts alone been removed. Under the microscope, the tumour presented the well-marked appearances of a round-celled soft melanotic sarcoma. Usually this kind of tumour affected the interior of the eye, but in the present instance, and another in which the eye (which was exhibited to the Society) was removed some time previously, the disease had originated externally.

5. *The President* showed (*a*) a specimen of RUPTURE OF THE DIAPHRAGM; (*b*) a specimen of RUPTURE OF ONE OF THE COLUMNÆ

CARNEÆ OF THE HEART. (*a.*) The specimen was taken from the body of a female who committed suicide by throwing herself over the Dean Bridge. The rupture, which was four inches in length, occupied the left side of the diaphragm, and allowed of the passage of the cardiac end of stomach into the left chest. The liver was superficially lacerated, and both thighs were fractured. She was alive when found, but died shortly after. (*b.*) In this case the rupture was found in the left ventricle. Externally there was a slight livid discoloration of the pericardium over the site of the rupture. Deceased was 50 years of age and of intemperate habits. He quarrelled with a man in a public house, and received some injuries to the face. The two in struggling fell to the ground. Deceased was assisted home and left alone in the house. Next morning he was found dead, lying on the floor, and undressed. The heart exhibited, under the microscope, the usual appearances of fatty degeneration.

V. ORIGINAL COMMUNICATIONS.

1. SOME LOCAL CAUSES OF FŒTID BREATH.

By P. M'BRIDE, M.D., F.R.C.P. Ed., F.R.S.E., Surgeon to the Ear and Throat Department of the Royal Infirmary, Edinburgh, and Lecturer on Diseases of the Ear and Throat, Surgeons' Hall.

FŒTOR of the breath is a symptom which may occur under such a variety of circumstances that a full consideration of the subject would carry one far beyond the limits of a paper.

The expired air may acquire a disagreeable odour from pulmonary or laryngeal disease, from mineral poisons, from ulcerations of the mouth, or from the presence of decayed teeth. All these and other conditions to which the symptom in question may be due are doubtless well known to all present.

There are, however, three frequent causes of fœtid breath which are perhaps less fully understood, and which may therefore prove of interest to members of this Society, namely—

(1.) Follicular Tonsillitis (Chronic).
(2.) Ozæna.
(3.) Catarrh of the Tongue.

Chronic Follicular Tonsillitis.—In this affection the crypts of the tonsil are the parts which are most affected. The mucous membrane which lines them pours out a secretion which tends to become inspissated and afterwards to decompose. As a result the appearance on examination is that of a white patch situated on the surface of the gland. If the adjacent part of the latter be pressed upon, a white pellet is extruded, composed of decomposing organic matter, and having consequently a fœtid odour. When the pathology of this condition is once understood, the indications for treatment become plain. When the crypt has been emptied of its con-

tents an unhealthy or catarrhal state of its lining membrane remains, and probably the best application is the introduction of a thin probe which has been previously covered with nitrate of silver. Attention to the general health and to the adjacent mucous membrane may also in some cases expedite the cure.

Let us now turn to another and more important affection, viz.—

Ozæna.—The term ozæna is used in two distinct senses. To some it signifies a definite disease characterized by certain well-marked pathological and clinical features, of which however ulceration is not one. By others any fœtid discharge from the nostrils is spoken of as ozæna; or, in other words, the name is applied, not to a definite morbid condition, but to a symptom which may occur in the course of various diseases. Thus, syphilitic and, according to some, scrofulous ulcerations of the nose may in themselves produce a flow of fœtid pus, or they may be the immediate causes of caries and necrosis of the nasal bones. Then, again, an impacted and perhaps long forgotten foreign body, or a nasal calculus, may be the origin of the symptom in question. Primary malignant disease of the nasal cavity and glanders, both rare affections, are also associated with ozæna when used in this less exact sense. Strumous ulceration of the nasal mucous membrane is rare except in childhood, and its very existence seems to be called in question by some. Syphilis, on the other hand, if present, can usually be detected by means of a careful examination; yet every practitioner has met with cases of fœtid nasal discharge in which a specific taint could with certainty be excluded, and in which the condition was due to true ozæna or atrophic nasal catarrh, an affection which is much more common than most of the conditions alluded to, and which is rarely or never accompanied by ulceration. The object of my remarks so far has been to show how necessary it is to make a careful objective examination of the nasal cavity in every case by means of a speculum and reflected light, or, if necessary, by means of posterior rhinoscopy; and, having arrived at this conclusion, I shall enter into a more detailed account of true *ozæna or atrophic nasal catarrh.*

As the clinical features of this disease are well marked and admit of no dispute, I shall begin with a short description of the most prominent of these. The affection usually begins in early life, most commonly about the age of puberty, and the first symptom to attract attention is usually a fœtid smell observed, not by the patient, but by those who come in contact with him. An inspection of the nasal cavities by means of a bivalve speculum and reflected light is usually sufficient to establish a diagnosis. The most prominent characteristic noticed by the observer is extreme roominess, and a closer examination enables him to detect that this is due to diminished bulk of the turbinated bones—more particularly the inferior. Frequently the view is obstructed by quantities of gray or greenish crusts, which can, however, be washed

away by means of a spray, while again in other cases there is little evidence of excessive secretion. It is true that one or two cases have been reported in which no such evidence of atrophy of the mucous membrane could be detected; but, as Mackenzie remarks,[1] "Limited atrophy might easily have existed in some situation not accessible to view." As to the cause of this atrophy, the weight of evidence seems to show that it is usually secondary to hypertrophy, the result of chronic inflammatory thickening. During the latter process, I take it, the mucous membrane is infiltrated with quantities of round cells, which, however, tend to lengthen into spindles; gradually this change results in the formation of a quantity of fibrous bundles, which by their pressure cause atrophy of adjacent tissues, including the spongy bones.[2]

As to the cause of the marked fœtor, which is the most distressing and prominent feature of the disease, opinions differ, and many theories have been advanced. Thus, Michel (*Krankheiten der Nasenhöhle*), by an ingenious line of argument, based principally on a process of exclusion, advances the hypothesis that the objectionable odour is due to the presence of decomposing secretion in the sphenoidal and ethmoidal cells. While, in a very small minority of cases, this condition may exist, the great bulk of pathological evidence is against this view,[3] which is, however, also held by Bosworth.[4] Krause,[5] as a result of two post-mortem investigations, states that in addition to a fibroid and consequent atrophic process, which tends to destroy the gland structures of the nasal mucous membrane, there is an enormous infiltration of round and spindle cells which show marked evidence of fatty degeneration; and to the resulting formation of fatty acids he ascribes the fœtor. In the discussion which followed, however, E. Fränkel, of Hamburg, stated that in three cases observed by him the fatty condition was absent. Again, other observers believe that atrophy of the glands of the mucous membrane is the immediate cause of the symptom in question. Probably the most satisfactory view in the present state of our knowledge is that expressed by B. Fränkel, of Berlin, who maintains that it is necessary to assume the presence of a specific ferment in cases of ozæna to meet the fact that atrophic catarrh sometimes, though rarely, exists without fœtor. The last theory to which it is necessary to allude is that advanced by Zaufal, that the tendency to decomposition is due to diminished growth of the turbinated bones, and the widening of the nasal passages consequent thereon, which diminishes the force of the respiratory current of air, and tends thus to favour the retention of secretions. That there is a certain element of probability in this

[1] *Diseases of the Throat and Nose*, vol. ii. p. 329.
[2] *Zuckerkandl Normale und Path. Anat. der Nasenhöhle*, p. 87.
[3] Compare Zuckerkandl, *op. cit.*, pp. 93, 94.
[4] *Diseases of the Throat and Nose*, p. 227.
[5] *Transactions of International Med. Congress*, 1881, vol. iii. p. 312.

view will be evident to all; but that the widening of the nasal fossæ is due to retarded development alone is extremely unlikely.

A consideration of all these views then leaves us only in a position to say that ozæna is associated with atrophy of the nasal mucous membrane. If the secretion be examined in its fluid state, it is found to consist of pus cells and enormous quantities of bacteria.

The predisposing causes of ozæna are still uncertain; for while some authors have found this atrophy of the nasal mucous membrane to originate more frequently in those of a strumous or specific constitution, Mackenzie[1] says, "In adults dry catarrh shows no special disposition to affect the strumous. I have met with only three cases in which there was any evidence of hereditary syphilis, and I only know of three in which ozæna, without ulceration, has followed acquired syphilis."

For the treatment of ozæna an enormous number of drugs and applications have been suggested. Attention to the general health should of course, in many cases, accompany local medication, but to the latter we must look as our sheet anchor.

There can be no doubt that the most effectual method of treatment consists, in the first place at least, in thorough cleansing of the whole nasal cavity; and although douching and syringing are much used, I do not believe that these can be at all compared with the anterior nasal spray, as used at the London Throat Hospital, either in point of efficacy or safety. While agreeing with those authors who believe that ozæna is often incurable, yet I am also of opinion that in most cases the fœtor, which is after all the only symptom calling for interference, can be kept in check for an indefinite time by the methodical use of appropriate sprays. According to my own experience, the most satisfactory results are derived from the use of an alkaline and antiseptic spray, immediately followed by one containing as much tincture of iodine as the patient can bear, which usually does not exceed five minims to the ounce. The alkaline spray, of course, washes out all parts of the nasal cavities thoroughly, and leaves a perfectly clean surface for the remedial effects of the iodine, which probably acts in its double capacity of antiseptic and stimulant. Some authorities prefer to apply their remedies as powders, and the most important of these latter are iodoform, boracic acid, and eucalyptus (diluted with starch); while others resort to a gelatine bougie as a vehicle for introducing medicinal agents into the nares.

Gottstein, finding that it was only the dried secretion which gave forth the disagreeable characteristic odour, has suggested plugging the nares with cotton wool for a time varying from two to twelve hours, and this method may be worthy of imitation in cases where the spray treatment has failed. The inhalation and ejection through the nose of creasote vapour may also be followed by

[1] *Diseases of the Nose and Throat*, vol. ii. p. 327.

benefit, and by relief from the feeling of dryness which is sometimes complained of. I do not know whether in obstinate cases the treatment may be aided by the internal use of sulpho-carbolates, although, arguing from analogy, one might expect a beneficial effect from the employment of internal antiseptics.

The next, and I believe by far the most common, cause of fœtid breath is catarrh of the tongue.

Catarrh of the Tongue.—Furring of the tongue is so common and so frequently associated with robust health, that its pathological nature has been almost lost sight of, except, indeed, when it occurs as a symptom of general disease. Yet the frequent catarrhal origin of this condition cannot, I think, be denied, although at first sight the objective appearances are somewhat unlike those usually presented by inflamed mucous membranes. In the œsophagus, however, it would seem that chronic superficial inflammation tends to produce a desquamative process analogous to that so often seen in the skin;[1] and when we consider the analogy of structure between the mucous membrane of the gullet and that covering the tongue, and, moreover, the fact of the intimate relation which subsists between these two organs, it is difficult to avoid the conclusion that a similar analogy will subsist between their pathological conditions. Another strong argument in favour of the inflammatory origin of furred tongue, if further proof be needed, is the fact that during local acute affections of the tonsils, pharynx, etc., where the whole mucous membrane participates to some extent, the tongue is always thickly coated. All this notwithstanding, we find little allusion to tongue catarrh in any of our standard works. Among specialists, Michel alone, so far as I know, refers in a footnote to the differential diagnosis between this condition and ozæna,[2] stating that he has observed it as a cause of fœtid breath; while Niemeyer[3] writes, in his article on "Oral Catarrh," as follows:—"The foul taste, or more correctly the foul smell, is not solely a subjective symptom; generally other persons perceive a fœtor from the mouth of the patient, especially in the morning before breakfast." Niemeyer, be it observed, speaks of tongue catarrh as a symptom of oral catarrh; but the fact to which I desire to direct attention is the great frequency with which it occurs alone, or at least without any evident catarrh of adjacent parts. Both among the laity and the profession fœtid breath associated with foul tongue is commonly ascribed to dyspepsia. The common connexion between the two conditions cannot be denied, but it seems to be too often forgotten that the fœtor is not due directly to the presence of noxious material in the stomach, but to decomposing inflammatory products on the back of the tongue. The fur in such cases is known

[1] Klebs, *Handbuch der Patholog. Anatom.*, vol. i. p. 158.
[2] *Krankheiten der Nasenhöhle und des Nasenrachenraumes*, 1876, p. 30.
[3] *Practice of Medicine*, vol. i. p. 417.

to consist of epithelium and mucus, and often contains bacteria. That fœtid breath is in many instances due to this condition may be proved by a process of exclusion arrived at by a careful local examination of the mouth and nose, and a knowledge of the fact that, except during deglutition and eructation, the œsophagus is firmly closed.

I may be met here with the argument that many cases of this kind can be cured by internal medication; but it must be remembered that this in the present instance is often also local medication, and is also generally accompanied by avoidance of irritating ingesta, a prohibition which acts in favour of both organs (stomach and tongue). This brings us, however, to another point of interest. It is manifest that furred tongue, when due to stomach catarrh, is not always produced by direct extension of inflammation from one part to the other, but in many cases probably owes its origin to a nervous connexion, and is therefore of the nature of a tropho-neurosis.

We do not yet know sufficient concerning vaso-motor connexions to enable us to trace the nervous chain of communication between tongue and stomach, but the anastomosis between the glossopharyngeal and pneumogastric naturally suggests itself to the inquirer.[1] If, however, it be generally admitted that the tongue sympathizes so readily with the stomach, then it almost follows that the converse must be equally true. The conclusions then which one is inclined to draw from these facts are:—

(1.) That fœtid breath, when due to dyspepsia, is caused by the accompanying catarrh of the tongue.

(2.) That local treatment directed to the tongue is the best method of meeting this symptom, and may also influence favourably any coincident gastric disturbance.

Catarrh of the tongue may, however, exist by itself. In such cases over-indulgences in tobacco, alcohol, or highly-spiced foods may be the direct exciting cause.

My attention was first directed to this matter by the following case:—A. W., æt. 40, a robust, powerful man, applied at the ear and throat department of the Royal Infirmary on account of discomfort in his throat, which he himself ascribed to an elongated uvula. He suffered constantly from a bad taste in the mouth, and his breath was offensive, especially in the morning. The only abnormality to be detected on examination was a thickly coated tongue, which was not easily accounted for, as the patient was not given to indulgence in alcohol, nor was he troubled with dyspepsia or constipation. His appetite, moreover, was good, and his conjunctivæ free from yellow tinge, so that I could not avoid looking upon this case as one of uncomplicated catarrh of the tongue. This view was confirmed by the result following the use of local remedies, for the patient returned a fortnight afterwards almost freed from his unpleasant symptoms.

[1] See Fairlie Clarke, *Diseases of the Tongue*, p. 10.

Before proceeding to discuss the subject of treatment, I must refer briefly to the furred tongue of fever, as this condition can in some cases hardly be considered catarrhal or even inflammatory.

During health there is a constant process of desquamation from the surface of the tongue, but the cast-off epithelium is washed away by the secretions of the salivary and other glands, so that, except perhaps at the back of the organ, a clear pink surface is left. Manifestly, then, furring may be caused either (1) by excessive production of epithelium, such as may result from a catarrhal condition; or (2) excessive dryness, which may be due in part to diminished activity of adjacent glands, and in part to rapid evaporation caused by increased body temperature. We can therefore only class as catarrhal those cases of furred tongue in which this is due to over-production of epithelium, and must exclude those in which it is caused by diminished function on the part of the secreting structures within the mouth, or by rapid evaporation, as seen in cases of nasal stenosis.

To the general treatment of these cases I need not allude; but I believe that a considerable amount of benefit may be obtained from the addition of topical applications. In the case which I have briefly described, a solution of nitrate of silver in nitrous ether, which is so valuable in cases of scaly eczema, was the remedy employed, while Michel advises the use of concentrated acetic acid.

I am aware that my remarks on this subject are not supported by a sufficient number of cases to make them conclusive, and that I have had to rely much upon theory; yet I trust that those who have opportunities of observing dyspepsia may be led to test their accuracy.

Dr Duncanson said he had listened with much interest to Dr M'Bride's paper. He had himself, some years previously, brought before the Society the method of treating cases of ozæna by applications of iodine and bicarbonate of soda. He used these in the form of a spray, worked by foot-bellows. The tubes for irrigating the nasal chambers he had at that time to make for himself. After thoroughly cleansing out the nostrils he applied powdered iodoform, which was then new to most Edinburgh practitioners. Along with constitutional treatment he had found this most efficacious. He should like to ask Dr M'Bride whether his cases were nose-breathers or mouth-breathers. He thought this was a very important point, and he taught his patients to breathe through the nose. George Catlin's book, *Shut Your Mouth*, he thought was most instructive, and always recommended it to the attention of his students. Cases of one-sided ozæna, he had found, were not unfrequently due to the presence of a foreign body. In one instance he had removed what appeared to be a

papier-maché button from the nostrils of a boy suffering from this affection, with the result that he completely recovered.

The President said he was reminded, by Dr Duncanson's remarks, of a case that occurred to him when a country practitioner. It was a young girl with one-sided ozæna. She had been to several doctors, but had not derived much benefit. He fortunately detected and removed a foreign body, which he sent to Bennett, then Professor of Physiology, for microscopic examination. It was reported to be a piece of shaving, and the probability was that the girl had put it up when a child playing about some houses that were then being built.

Dr Clouston asked if Dr M'Bride would give them his views as to the possible neurotic origin of ozæna. It sometimes occurred in the younger members of families in which epilepsy, insanity, and allied affections were hereditary. He should like to know if it was not possible that it might be a trophic neurosis. If that was the case, ozæna might be hereditary without being due to a syphilitic infection. He had in his eye three generations in which it appeared, where the chance of such infection was very improbable.

Dr M'Bride, in reply, said he thought that excessive space for nose-breathing was commonly associated with ozæna. The air passing over the secretions dried them, so that they became hard, firm, and adherent to the mucous membrane. He did not think that one-sided ozæna was usually due to the presence of a foreign body. There was in such cases as he had seen an atrophic condition of the nostril present just as in the ordinary form of the malady. In regard to Dr Clouston's question, his observations certainly seemed to point to the probability of ozæna being a tropho-neurosis, but in such cases as those mentioned the possibility of a hereditary syphilitic taint must always be kept in view. He was aware that ozæna was sometimes hereditary, and that possibly without specific taint. At the same time, he was not aware of any observations pointing to an association between ozæna and marked hereditary neurosis.

2. CASE OF EMPYÆMA, WITH OPERATION FOR CLOSURE OF PLEURAL CAVITY BY REMOVING PORTIONS OF FOUR RIBS.

By A. G. Miller, M.D., F.R.C.S. Ed., Surgeon to the Royal Infirmary, and Lecturer on Surgery, Edinburgh.

Report of case on admission kindly furnished by Dr Wyllie.

D. S., æt. 22, single, floorcloth printer, admitted into Ward 31, Royal Infirmary, on 12th April 1883, complaining of shortness of breath, cough, and general weakness.

History.—Had always been a strong healthy man till his pre-

sent illness, which began, three months before his admission, with severe pain in the left side. Says that the room that he worked in was close, ill-ventilated, and dusty; and thinks his illness was due to cold, caused by a draught in the room. The pain began severely one morning when the patient awoke. He consulted a medical man, who diagnosed pleurisy, and tapped the chest about the end of the first week of the illness. At the operation, 2½ pints of clear fluid were withdrawn, and the patient was much relieved, though he states that much cough, with frothy expectoration, followed the operation. He was tapped a second time six weeks afterwards, but does not know what quantity of fluid was then taken. The pain in the side had disappeared before the first operation, and has not since returned. There was no pain or discomfort after either of the tappings. There has all along been a short dry cough, and the illness has been attended with considerable loss of strength. The family history is good. The father died from accident, the mother from cancer of the mamma. The brothers and sisters are all alive and well.

Present condition.—The patient is well formed; height, 6 feet; weight, 12 stones. Looks pale and delicate, and says he has lost weight considerably during his illness.

There is some fulness of the left side of the chest, which measures at the level of the nipple 18¾ inches, as compared with 18¼ on the right side. On physical examination, it was evident that air was contained in the upper part of the left pleural sac, and fluid in the lower part, the dulness produced by the fluid having its upper limit about the level of the lower angle of the scapula behind, and about the level of the nipple in front. Hippocratic succussion could easily be produced, and the patient was conscious of a splashing in his side when he turned himself in bed. Over the upper or resonant portion of the left side there was some very faint and distant respiratory murmur of a vesicular type, accompanied, both in front and behind, by a few metallic tinklings. The vocal resonance and fremitus were much diminished. Over the lower or dull portion there was complete absence of breath-sounds, as well as of vocal resonance and fremitus. On the right side the percussion note was normal, and the respiratory sounds loud and harsh, but otherwise normal.

The heart was evidently pressed on to some extent, the apparent apex beat being in the epigastrium, and the right border of the cardiac dulness being 2 inches to the right of the sternal margin. The *appetite* was fairly good. The patient *slept* fairly well. The *urine* was normal. The patient's *respiration* as he lay in bed was moderately quiet, the number of respirations per minute varying from 24 to 32. His *pulse* was weak, rapid, and easily compressible, 108 to 134 per minute. His *temperature* presented a considerable amount of diurnal variation, the morning temperature being 98° to 99°, and the evening temperature 100°·5 to 102°.

On the 15th of April, three days after the patient's admission, as the rapidity of the pulse and respiration was increasing, the chest was tapped with an aspirator, and 34 ounces of pus withdrawn. The pus was not fœtid. The patient felt better after the operation, but as the case was thought one that would need to be treated by incision, the patient was, on the 17th April, transferred to the surgical wards, to be placed under the care of Mr Miller.

Surgical History.—Antiseptic opening of the pleural sac was performed in the beginning of May, and a large quantity of pus evacuated. The incision was made below the angle of the scapula in the ninth intercostal space. As drainage was not perfect on account of the rising of the diaphragm, a second opening was made on 20th July, by removing a portion of the ninth rib.

Operation.—Considerable difficulty was found in administering chloroform, as the patient had to be laid on his right side, and partly on his face, to admit of the necessary manipulations, and this materially interfered with the expansion of the right lung, on which he had to depend for respiration. An incision was made at once down to the rib immediately above the previous wound, the rib exposed, the periosteum separated, and about an inch of the rib removed with the bone pliers, after being partially sawn through with a Hey's saw. There was little bleeding, and a knife was then pushed through the posterior layer of periosteum and costal pleura, and a permanent opening established by inserting a large-sized drainage tube. At this operation two drainage tubes were recovered which had previously been sucked into the cavity, notwithstanding ligatures having been attached to them. On examination with the finger, the pericardium and contracted left lung were easily felt, both covered with granulations; the latter apparently firmly tied down to the vertebral column. After this operation patient improved very much for some weeks.

Oct. 5th, 1883.—Examination by Dr Wyllie. Chest, right side, 18¼; left, 16—together, 34¼; left side somewhat shrunken. Anterior wall flattened, and movement very defective on left side. Percussion moderately resonant over whole left side, least so over base near wound. The note is nowhere quite so full as the normal percussion note of the right side. "Bell sound" beautifully present over whole side, except in the scapular region, where thickness of the parietes probably interferes with it. With wound closed there is absolute silence over whole left side on auscultation; but if air is allowed to enter at the wound it produces a sound which, on auscultation of any part of the side, is heard as a loud ringing amphoric breathing. The right side is normal, except that the breathing sounds are puerile.

Dec. 14th, 1883.—Carbolic gauze changed to boracic lint, as former was irritating the skin. From this period, eight months after opening, up till March 1884, no record was made in the case

book. By the latter date the use of the spray and antiseptic precautions had been discontinued on account of the discharge having become putrid. At this period the dressings were of carbolic or salicylic wool changed almost daily. Sometimes as much as ℥xx. would come away in twenty-four hours. After this the patient began to lose ground progressively and perceptibly.

On 30*th May*, 1884, an operation was performed to promote falling in of the chest wall, and, if possible, obliteration of the extensive suppurating cavity. About an inch and a half was removed from four ribs, the 4th, 5th, 6th, and 7th, by a procedure similar to that previously described, only that a chisel was used instead of the Hey's saw. Each portion was removed by a separate incision made over the long axis of each rib in the anterior axillary line. This position was made necessary by the impossibility of laying the patient on his right side.

These portions of ribs were shown to this Society on the 2nd July last, when it was reported that their removal was somewhat difficult on account of the ribs being all closely jammed together. The portions removed were twice their normal thickness from deposit on the inner surface.

Soon after this operation the flattening of the chest wall visibly increased, the lower ribs (8th and 9th) standing out very prominently; and there was no space evident where the ribs had been removed.

Report of examination by Dr Wyllie.—*June* 18*th;* three weeks after operation. Circumference of chest at level of nipples, 33 inches; right side, 18; left side, 15. Much contraction on left side in anterior and lateral regions; very little in posterior region. During ordinary respiration, scarcely any expansion of left side except in lower posterior region. In upper anterior region there is movement of the clavicle during inspiration, but this seems to be altogether dependent upon the expansive movement of the right side, with elevation of the right clavicle and manubrium sterni. Box of larynx depressed very slightly, $\frac{1}{16}$ inch with each inspiration. Alæ nasi do not move at all. Percussion on left side behind markedly dull down to level of inferior angle of scapula, below which it is less dull, there being some boxy resonance in the percussion note. In front, percussion note quite dull above clavicle, and comparatively dull over whole of anterior and lateral regions; least marked between clavicle and upper edge of 3rd rib. *With two coins*, something of metallic clang can be heard in supraspinous region posteriorly; also over base below angle of scapula, but this "bell sound" is inaudible elsewhere in front or behind.

Auscultation. — Some vesicular breath-sound (not much) is audible over scapular and interscapular regions, but not elsewhere. Breath-sounds elsewhere absent.

Accompaniments.—None if wound be kept closed; but if air

be allowed to enter at wound, loud metallic tinkling râles are audible, both over apex in front and behind and over base close to wound.

Vocal resonance diminished, except over lung posteriorly. Nowhere any metallic echo.

Right side percussion note fairly good all over.

Auscultation.—Breath-sounds harsh and vesicular, with prolonged expiration over the whole side. Bronchitic accompaniments, especially sibilations, present over whole side, but in no great abundance. Vocal resonance very distinct, but not louder than might be expected from emaciated condition of costal parietes.

After this period discharge diminished considerably, and there was promise of the wound closing, and the patient improved in general health and spirits. Injections of various lotions were tried to hasten the closure of the cavity and approximation of its walls. Iodine caused some irritation and increased the discharge. Corrosive sublimate (1 to 2000) seemed to do well, but had to be discontinued on account of its producing diarrhœa. Twice the wound was closed and sealed with collodion after careful washing out, but the discharge was merely dammed back, and had to be let out after a few days on account of the respiration being impeded.

The cavity which before the removal of the ribs contained many ounces of fluid, never could be made to retain more than 3 ounces of injection after the operation.

For a week or two before his death the patient suffered from fits of unconsciousness, which afterwards were accompanied by convulsions, sometimes unilateral and sometimes bi-lateral. In Dr Wyllie's absence he was seen by Dr Muirhead, who pronounced them to be due to uræmia.

Patient gradually sank and died on the 11th September 1884—one year and five months after the commencement of his illness.

Post-mortem examination made on September 13th, 1884, by Dr Alex. Bruce, in the absence of Dr Bramwell.

Chest.—Left side of the thorax below the level of the nipple is much contracted. The right side of the chest, at the level of the base of the xiphisternum and also that at the level of the second rib, measures 16½ inches. The corresponding measurements on the left side are 14 inches and 14½ inches.

On the left side there is a sinus pointing between the 8th and 9th ribs, and another between the 9th and 10th ribs, also four linear cicatrices over the 4th, 5th, 6th, and 7th ribs, 2½ inches long, crossing the anterior axillary line on the left side.

Heart.—Apex found lying between the 3rd and 4th ribs, 5 inches to the left of the middle line.

In the left mammary line the upper border of the diaphragm extends up to the lower border of the 4th rib.

The left pleural cavity contains 7 ounces of cream-coloured, not fœtid, pus. The fluid occupies less than a fourth of the cavity. The right pleural cavity is partly obstructed by adhesions. The left pleural cavity extended from the level of the lower border of the 4th rib upwards to the apex. This cavity communicated by an aperture ¾ of an inch in diameter with a smaller cavity capable of containing 2 or 3 ounces of fluid. This second cavity lay behind the apex of the heart, and communicated with the external openings noted above. The parietal pleura of the upper cavity is much thickened and granulated. The left lung is contracted to a mass slightly larger than a kidney. The right lung was found to be healthy. The liver weighed 8 lb., and was indented by the ribs. It was markedly waxy. The left kidney weighed 10 oz.; the capsule was slightly adherent, congested, fatty, and waxy.

Remarks.—The case was one of *pyo-pneumo-thorax*, following tapping for effusion after pleurisy.

(*a.*) I would remark, in the first place, that aspiration was unequal to the task of permanently removing the pus from the pleural cavity, even though the fluid was apparently aseptic. The only test applied to the pus was the microscope. No bacteria were seen; but the belief that the fluid was aseptic is supported by the fact that when it became distinctly septic nine months after, the patient suffered from septic fever, and then multitudes of bacteria were seen in the discharge.

(*b.*) The stages noted in regard to the falling in of the chest are worthy of close attention. The measurements were, when he came in, 18¼ inches right and 18¾ inches left, the diseased side being the larger. Six months after admission, the measurements were 18¼ inches and 16 inches. This showed that collapse of the chest wall had progressed as far as it could; and this was proved at the second operation by the ribs being found in absolute contact. After this operation and the removal of portions of four ribs, the measurements sunk to 18 inches and 15 inches, showing still greater collapse. But besides the diminution in girth there was marked flattening over the part where the ribs had been operated upon. It will be seen, from the preparation lying on the table, that adhesion has taken place between the contracted lung and the chest wall just where the ribs were removed. And I think it is not too much to suppose that had portions of more ribs above, and perhaps also of the clavicle been removed, and still more had this been done a year sooner, complete closure of the cavity would have resulted and the patient's life been saved. An earlier operation was prevented by the obstinate refusal of the patient to submit to any interference.

(*c.*) As to the operation, I consider that if performed subperiostially, antiseptically, and by a separate incision for each rib made lengthwise, there should be no danger of life whatever, even

though portions of seven or eight ribs were removed and part of the clavicle also. When I operated on D. S., he suffered very little from shock, though he was very weak at the time. The steps of the operation are simple—1st, an incision in the long axis of the rib for say 3 inches, carried down to the bone at once; 2nd, the periosteum separated carefully; 3rd, the rib is then cut half through with a Hey's saw or a chisel, and its section completed with the bone pliers. There should be no hæmorrhage, for the intercostal artery is safe from injury outside the periosteum. At the attachment of the intercostal muscles, a few touches of the knife may be required.

(*d.*) Now as to the part of rib to be removed, I would recommend the anterior axillary line for the following reasons—1st, That part can be easily got at without having to turn the patient over on the opposite side, which necessarily impedes breathing, and interferes with the safe administration of an anæsthetic; 2nd, The ribs are thinner in front than behind, and more easily cut through; 3rd, The operation can be performed separately from the incision for drainage and dressed separately.

(*e.*) With regard to the question of drainage, I have found that a very dependent opening made at first, as in D. S.'s case, is not the best, because after the opening the chest collapses, and the diaphragm rising acts as a valve, preventing free flow from the cavity. The high level to which the diaphragm rose in D. S.'s case is worthy of notice, being 2 inches above the normal level. I would imagine, therefore, that an opening between the 7th and 8th, or 8th and 9th, ribs below the angle of the scapula would make the best drain.

(*f.*) In referring to the history of the case I have recorded, I would use it as an argument for early operation. I am as sure as one can be of anything that D. S.'s life would have been spared had I operated a year sooner. Because, 1st, He died of amyloid degeneration; 2nd, He greatly improved after the operation, weak as he was; 3rd, The man was in fairly good health when he came into my ward, and would then have stood a much more severe operation than the one I performed a year afterwards; 4th, The parts would probably have contracted more thoroughly at an earlier period; 5th, The operation being aseptic, if performed at first, there would have been less risk at that earlier period; 6th, The lung might have expanded and materially assisted to fill the reduced cavity.

(*g.*) I have but one remark more to make, and it is in regard to the alterations discovered, post-mortem, that had resulted from the operation:—1st, The cavity had become virtually obliterated opposite the part of the chest wall operated on; 2nd, Further obliteration was prevented by the rigidity of the chest wall in the upper part. The ribs being in contact and the lung tied down, it was impossible for that portion to be filled up.

I would now conclude this hasty sketch by repeating the main points demonstrated by the case:—1st, The removal of a portion of a rib is a simple and safe operation; 2nd, The removal of portions of several ribs is not dangerous to life; 3rd, The anterior axillary line is the best point for operating, because there the ribs are thinner, and the operation can be performed with the patient on his back; 4th, A separate short incision for each rib is better than one long one in the long axis of the body; 5th, Removal of portions of ribs greatly assists the contraction of an empyæmic cavity, and if a sufficient number are removed from the proper ribs, such a cavity may be permanently closed; 6th, Such an operation should be performed early to give the patient the best chance while the health is fairly good, and there is a possibility of the lung expanding somewhat. I would be inclined to fix the time at that period when it has become evident that a simple drainage opening is not sufficient to effect a cure.

Dr John Duncan did not think any one would be inclined to question the conclusions to which Mr Miller came. In cases of empyema, there were a considerable number, so far as his experience went, in which the most absolute perfection of drainage was unable to complete the closure of the cavity. He had treated a large number of cases of empyema by single openings, and by double openings, and he thought that one might say that in neither way could they rely on the cavity closing without further operation. The consequence was that later on the patient died of amyloid degeneration. This operation of excising portions of the ribs to complete the closure of the cavity had now been frequently practised. According to some, with a very great degree of success; according to others, with but slight success. His experience lay between the two. In many of his cases he found the cavity diminished to a certain extent and then stopped. That, if they considered the mere mechanics of the subject, was what they would be inclined to expect. In one instance he had removed three inches in length from each of five ribs. They fell together with remarkable rapidity, but they soon fell no more. The only thing they could rely on in extreme cases was to take out a portion of each rib all the way down. In the case of a small cavity a few ribs might suffice. It must be remembered, however, that the taking away of portions of many ribs added greatly to the danger of the operation. Taking away parts of the upper ribs was a very different thing from removing parts of the lower, because there were certain anatomical structures in their neighbourhood which it might be unpleasant to meddle with. Still it was a question to be considered, whether they should not recognise it as a necessity, if they had to treat a large empyema, to do something as great as that, because the patient, if left alone, would die eventually. He had not yet ventured to do this.

Perhaps repeated operations might be safer, and more successful than a single great one.

Dr Byrom Bramwell said he had for some years been in the habit of teaching that where the lung was so bound down by adhesions that it could not expand, and where the chest-wall was so rigid that it could not collapse to meet the lung and fill the vacuum, it was necessary to remove portions of the ribs by excision. A much more important point, however, in his opinion was the prevention of the fixation of the lung by adhesions. With this object, it was of the greatest importance to determine, as early as possible, whether an effusion was purulent or simple. The ordinary symptoms (rigors, sweatings, etc.) were not always conclusive; but there was one means of diagnosis which was quite simple and safe, viz., the puncture of the chest by a hypodermic syringe. Whenever a copious effusion lasted for more than a few days without absorption occurring, then diagnostic puncture should be performed, for simple and purulent effusions require totally different treatment. A purulent effusion in the adult was practically never cured by simple aspiration, though it was sometimes in the child. A purulent effusion in the adult required to be treated either like a large internal abscess and opened under strictly antiseptic precautions by incision (this was the method which Dr Bramwell had been in the habit of teaching as advisable), or the pus should be drawn off by aspiration and replaced by an antiseptic solution. This recent method of treatment seemed to be preferable in many cases to the old method of incision.

Dr James said that, as a physician, he must homologate Dr Bramwell's remarks. He preferred to deal with cases before necessity for such operations arose, but when such cases came before them operation was called for, and he agreed with much that Dr Duncan had said. At the point where the portions of ribs were removed the lung and chest-wall usually came into contact. Successive operations might then be made at parts where they were not in contact and where a cavity remained, and in this way its closure would be assisted.

Mr Miller had much pleasure in listening to the remarks made on his paper. He thought Dr James's point a valuable one. One of the main objects of the paper was to try and disabuse certain minds of the idea that the operation was very serious. It was one that ought not to be delayed under the idea that it was dangerous.

Meeting II.—December 3, 1884.

Dr Littlejohn, *President, in the Chair.*

I. Election of New Members.

The following gentlemen were elected Ordinary Members of the Society:—Thomas R. Scott, M.B., C.M., Musselburgh; R. Milne Murray, M.A., M.B., C.M., F.R.C.P. Ed., Edinburgh; A. Murray Gibson, M.D., Portobello; A. S. Cumming, M.B., C.M., Edinburgh; Ernest F. Neve, M.B., Edinburgh; W. C. Greig, M.B., C.M., Edinburgh.

II. Exhibition of Patient.

Professor Chiene showed a woman, æt. 32, on whom he had performed the operation of œsophagostomy. She suffered from a tumour situated behind the box of the larynx, which it was at first hoped was specific, but anti-specific remedies had failed to produce any effect. When she was unable to swallow and was dying of inanition, the operation of œsophagostomy was proposed in preference to gastrostomy. Mr Chiene thought he had made a slight improvement on the ordinary method of opening the œsophagus, by first incising the muscles,—the anterior fibres of the sterno-mastoid and the posterior fibres of the sterno-hyoid and sterno-thyroid. By so doing the access to the œsophagus was rendered much easier, as was also the stitching of the mucous membrane to the skin, the tension being reduced. The tumour had diminished since the operation was performed, and if she improved in weight an attempt might be made to remove it. She was easily fed by the new mouth in the neck, the only disadvantage being the want of saliva.

III. Exhibition of Pathological Specimens.

1. *Mr A. G. Miller* showed a specimen of anchylosed knee-joint removed by amputation from a boy æt. 17. Nearly three years before, the lad had met with an accident to his left knee from a waggon. This set up synovitis, which was treated in a country place in England by poulticing, and ended in suppuration and anchylosis. Subsequently erysipelas occurred, and one of the incisions made to let out the pus severed the anterior tibial nerve. As a result the anterior tibial muscles were paralysed. About nine months ago he came into the Infirmary anxious to have something done. He had previously had three operations performed in Sunderland. On admission there were two or three sinuses and extensive caries of the lower end of the femur. Mr Miller enlarged the sinuses, and removed the carious bone, using chisel, gouge, and spoon. This was succeeded by a return of the disease. He had

noticed in an American journal a recommendation that in such cases the anterior wall of condyles should be removed, as other operations always left a large cavity in the lower end of the femur which nature was incapable of filling up except in young children. He therefore removed the patella and anterior wall of the condyles, preserving the periosteum. In the course of the operation he found the head of the tibia involved as well as the lower end of the femur. He contented himself with simply scraping away the disease from the tibia, but he now regretted he had not treated it similarly to the femur, for caries returned in it and necessitated amputation. The operation, so far as the femur was concerned, had been successful, the tissues having remained healthy.

2. *The President* showed (*a.*) A DOUBLE UTERUS, one horn of which had been recently delivered of a dead child at the full time. (*b.*) The HEART, LIVER, and KIDNEYS from a fatal case of railway injury. These organs were extensively lacerated though the skin had not suffered.

3. *Dr Andrew* showed the BRAIN and CORD removed that day from a boy æt. 7, who had died in the Sick Children's Hospital five weeks after an injury to his sacrum. He was admitted ten days after the injury with typhoid symptoms, slight pain over the sacrum, tenderness down one sciatic nerve, and discoloration. A week after admission an abscess formed. It was opened, and found to be superficial. Ten days later a second abscess formed under the scapula, and a third on the left buttock. During all this time the temperature was 102° or 103° F. at night, and there had been profuse diarrhœa with dry tongue and typhoid symptoms. Later on came slight head symptoms, with threatening of meningitis. The typhoid symptoms were improving till the night of the 1st December, when suddenly he became cold, faint, and comatose, and died. On examining they found that the abscesses were superficial, that in the chest there was some slight pneumonia, and secondary abscesses in the kidneys. On opening the head they found some fresh lymph over the upper surface with the vessels enlarged, and on raising the brain a large mass of extravasated blood continued down over the whole course of the spinal canal; and on sawing the bone they found caries of the sacrum itself. The case was one of interest from the amount of blood extravasated without direct injury. So far as he could make out, he believed the blood passed upwards from vessels ruptured about the diseased bone.

4. *Dr Byrom Bramwell* showed the HEART of a young person who evidently at some time or other had had a general attack of endocarditis. The mitral valve was contracted. There were some small old vegetations on the aortic segments. On the tricuspid

valve there were also some small old vegetations, and the interest of the case lay in this. Right-sided endocarditis was thought to be rare. Dr Bramwell believed that view to be erroneous. In the majority of cases he had met with in which there had been a severe rheumatic affection, and in which there had been an examination within a short time after the attack, there was evidence of right-sided disease in the shape of minute vegetations on the tricuspid valve. This had important practical bearings. If the tricuspid was frequently affected, it must in most cases recover completely from the endocardial inflammation, for marks of disease were not usually found in later life. If tricuspid endocarditis could be recovered from, there was every probability that in many cases left-sided endocarditis was also curable.

5. *Mr Joseph Bell* showed a FATTY AND FIBRO-CELLULAR TUMOUR removed that day from the right parotid gland and region of the pharynx of a middle-aged woman. Externally the tumour had existed for several years, and she had been advised to have it removed; but the advice was not taken. When admitted she was observed to breathe with difficulty, and on examining the mouth a tumour was felt projecting into the pharynx. In the erect posture, and holding the head forwards, she could breathe easily; but in the recumbent position the breathing was much more difficult, especially when she fell asleep. This internal tumour was growing with great rapidity. It had not existed quite a year. The external tumour was lobulated, firmly adherent to the jaw, bound down by fascia and below the pes anserinus. He himself and some of his colleagues thought they could trace a connexion between the two tumours. He was also of opinion that the tumours were simple in character. The question was, What should be done? The giving of chloroform in such a case presented some danger, more particularly as there was a very large thyroid, which also rendered tracheotomy difficult. It was proposed to open the trachea first, to be prepared, lest the internal tumour should have to be removed through the mouth. This was accordingly done, and the operation on the tumour commenced from the outside, an incision being made over the parotid. The pes anserinus was pulled to one side, and the finger passed under the ramus of the jaw round the tumour, which was found to have a neck and to be connected with the internal tumour. A little traction easily shelled this out from the pharynx. Slight hæmorrhage followed. It was agreed to leave the tracheotomy tube in for at least twenty-four hours, and he had every hope that the patient would do well. *December 24th.* —Patient has made a rapid and complete recovery.

6. *Prof. Chiene* showed a CALCULUS removed by nephro-lithotomy The patient suffered from symptoms of stone in the right kidney for four years. Medicinal treatment gave no relief. An exploratory incision was made in the right loin. The finger passed in over the

anterior surface of the kidney felt a distinct elevation in the lower half of the organ, which was made out by aid of a fine needle to be a calculus. The incision was carried down to it, but it was not till it had been firmly grasped by a vulsellum that it could be removed. A drainage tube was inserted for two days. On the third day the urine ceased to come by the wound.

7. *Dr Charles W. MacGillivray* showed the contents of a DERMOID CYST removed from the frontal region of a young man's scalp. The interest of the case was that the cyst lay in a depression in the bony skull almost simulating meningocele.

IV. ORIGINAL COMMUNICATIONS.

1. A CASE OF POISONING WITH NITRATE OF POTASH, WITH HINTS AS TO THE CONDUCT OF MEDICAL PRACTITIONERS IN CASES OF SUSPECTED POISONING.

By HENRY DUNCAN LITTLEJOHN, M.D., F.R.C.S. Ed., President, Lecturer on Medical Jurisprudence, Edinburgh.

BY our *sponsio Academica*, and the unwritten law of medical ethics, the practitioner is forbidden to publish secrets confided to him in his intercourse with his fellow-citizens who have consulted him professionally. While the general rule may be thus stated, like all rules it has important exceptions. The medical profession has duties it owes to the State, and it is not bound, like some forms of priesthood, to inviolable secrecy, whatever may be the nature of the information communicated by our patients. A crime, *e.g.*, cannot be allowed to be perpetrated, so to speak, in our presence and with our cognizance during attendance on a household, and we must not, on any account, allow ourselves to be entrapped into being made unconscious confidants of some one who is bent on mischief. It is a trite observation that no situation can well be conceived of a more difficult or embarrassing a character than that of a medical man who, during a longer or shorter attendance, has the horrible suspicion dawning upon him that he is unwillingly assisting at the commission of a great crime. He may well shrink in such circumstances from the responsibility he feels thus thrust upon him, because, as we all know, the differential diagnosis of poisoning from disease is no easy matter, and to the very last he naturally fears his own judgment, and dreads the onus laid upon him of denouncing a criminal, and, at the same time, the possibility of making a huge mistake.

It is not therefore to be wondered at that in our criminal records we find instances of vacillation on the part of medical men in such awkward circumstances, involving unfortunate delay both as regards the unhappy patient and also the chance of the criminal being

brought to justice. I need do no more than refer to the well-known Wooler case of poisoning, in which a lady suffering from a gastro-enteritis, which ultimately proved fatal, was found to have died from the slow administration of arsenic. For six weeks the symptoms dragged their weary course, with occasional exacerbations, before the three medical men in attendance came to an understanding that poison was being insidiously given to the lady. They did their best by chemical analysis of the urine to confirm their diagnosis, but in this they failed, and in their difficulty they applied to Sir R. Christison, to whom they sent a specimen of the urine. Meanwhile the conduct of the husband was most suspicious, and to him they communicated their opinion that the food of Mrs Wooler was being tampered with. He expressed his surprise at the statement, and it was noted that for some days thereafter marked improvement occurred in the symptoms, but these again became aggravated, and, unfortunately, Sir R. Christison's corroboration of the diagnosis only arrived on the forenoon of the lady's death. A lengthened trial took place. The evidence of the husband's guilt was not satisfactory to the jury, and a verdict (according to English law) was returned of "Not guilty." One of the three medical witnesses was a qualified assistant, who, when examined in court, stated that at an earlier date than the one just mentioned he had begun to suspect foul play. The counsel for the defence at once asked him, "What did you do when this suspicion crossed your mind? Did you communicate with your master?" "No." "Did you adopt any means to save the lady's life, and thus prevent the occurrence of a great crime?" "Did you," he was finally asked, "did you not mention the matter to the tenderly attached husband?" Unfortunately, as we must all agree, the assistant for the time kept his suspicions to himself. It is to be regretted that he made this admission in the witness-box, for his duty in the circumstances was plain, and he should at once have communicated his suspicions to the gentleman whose assistant he was, and thus have thrown upon him the responsibility of further action in the matter.

Should, however, a medical man find himself the sole attendant in a household, without an opportunity of consulting with another practitioner, and should he have occasion to suspect the cause of his patient's illness, What should be his line of conduct? Of course his first and paramount duty is to save his patient's life; his second is to warn the authorities of the existence of a criminal. How is he to accomplish these two objects? It is easy for us, sitting in a Society like the present, to lay down rules as to medical conduct, but such cases are puzzling in the extreme, and the circumstances are often such as to throw us off our guard, or, finally, events may progress with such rapidity as to prevent us being able to save life. In the Wooler case, the medical men, after giving the husband warning of their suspicions, administered anti-

dotes by the mouth and by the bowel, but they took no other decisive step, waiting as they did for the authoritative opinion of Christison.

In the Pritchard case, on the other hand, Dr Paterson, when hurriedly summoned to see Mrs Taylor, the mother-in-law, and subsequently Mrs Pritchard, found the husband, a duly qualified practitioner, in attendance, and he did not like actively to interfere beyond prescribing for the apparently urgent symptoms. He refused to furnish the registrar with a certificate of death in the case of Mrs Taylor, and pointedly drew the attention of the registrar to the mysterious nature of the death. In both cases Dr Pritchard ultimately gave the medical certificates, and these cases of murder by poisoning might never have been investigated, had not an anonymous letter reached the authorities with the signature "Amor Justitiæ." It has never been ascertained who the writer was. Dr Paterson denied that he was, and afterwards manfully brought an action in a court of law against a person who charged him with being a "common informer." Now I am clearly of opinion that we, as professional men, should have nothing to do with anonymous communications. It is a chance that such documents are attended to, and they might be allowed to fall aside. Here, again, the duty of the medical man in my opinion is plain. He should call on the public prosecutor and state to that official verbally his suspicions. On these being stated, action will be taken. No one can legally call in question the conduct of the procurator-fiscal in his criminal inquiries, and therefore all actions for damages are out of the question, and the name of his informant is never divulged. Both in town and country these officials can easily be found, and there need be no difficulty in communicating with them.

I have been consulted in these difficult and trying cases, and such has been the advice I have invariably given. In many of them the suspicions have proved to be groundless, the inquiry has been judiciously conducted, and no one has ever learned who it was that put the officers of justice in motion.

The case which I want to bring under the notice of the Society to-night refers to the action of the authorities after suspicion of a grave character has attached itself to an individual. Were he to be taken into custody to await the result of a post-mortem examination, and were the dissection to disclose nothing of a criminal or suspicious character, the suspected party by his very incarceration has sustained an irreparable blow to his character, and it behoves medical men, therefore, to be careful in their diagnosis when such results may follow the mere expression of their opinion.

On a Friday evening I was waited on by a woman and an elderly man, who said he was her landlord, with a request that I would grant a certificate to the registrar as to the death of the

child of the woman, which had occurred somewhat suddenly that afternoon. The parties were evidently in poor circumstances, and I at once agreed to do so, if I were allowed to make a post-mortem examination of deceased. This the mother refused to allow, and her manner in doing so was so peremptory and peculiar that I determined to mention the case to the acting lieutenant of police that evening, and request him, on behalf of the authorities, to make some inquiry into the circumstances of the case.

This I did; and as I was speaking to the lieutenant, two women made their appearance and detailed what seemed to them suspicious facts as to the sudden death of a child in their immediate neighbourhood. I at once recognised the case I had just been reporting, and the following statement was obtained from the witnesses, viz.:—That the child was illegitimate, and that the mother had been heard to complain of the burden of its maintenance, and to speak harshly to it; that the child was a remarkably fine healthy boy, and a favourite among the neighbours. That that afternoon it had been taken out by its mother and brought back in the course of half an hour ill and vomiting, and shortly after they were surprised to hear of its death.

The lieutenant consulted me as to the best course of procedure. Should the mother at once be taken into custody, or should the police delay taking this step till further investigation next day? Two risks were apparent,—it was evident that delay would give the suspected party full opportunity of disposing of and obliterating, as far as possible, all vestige of her crime. On the other hand, it was possible that the death might be due to natural causes, and that a post-mortem examination might at once clear up the case. Under these circumstances, prematurely arresting the mother would inflict a grievous stain on her character, of which she might never be able to get rid, and thus she might suffer a great injustice. The only way out of the difficulty, and which I beg to recommend to be followed in similar circumstances, was the plan ultimately adopted. A detective officer in plain clothes was instructed to proceed to the house where the deceased lay, and there remain during the night, narrowly watching the conduct of the mother, and taking special care that no article was removed from the house, and that nothing in the way of washing articles of clothing, or even the floor of the apartment, was permitted. Next morning the public prosecutor, on being made acquainted with the circumstances of the case, and with the action of the police, expressed his approval, and at once gave instructions for the examination of the body of the child, associating with me in the inspection the parochial medical officer of the district, to whom it was ascertained the mother had proffered a similar request to certify the death after my refusal. The following is the report which was furnished to the authorities:—

Report of the Post-mortem Examination of the Body of William Walker.

We hereby certify, upon soul and conscience, that on the forenoon of Saturday, we examined, at No. 3 Greenside Lane, the body of a male child, which was identified by Alexander Innes, Isabella Gould or Innes, Mary Ross or Jack, Margaret Berry or Lang, and also by Elizabeth Smith or Walker, as that of William Walker.

External Appearances.—The body was that of a healthy, well developed male child about 3 years of age. The body was dressed for the grave; it was fresh, and presented no marks of external violence. The usual post-mortem rigidity and lividity were present.

Head.—The cranium was intact. The brain and its membranes were much congested, but were otherwise healthy.

Mouth, etc.—Nothing unusual was observed in the mouth, throat, or upper part of gullet. There was slight congestion of the mucous membrane at the lower end of the gullet where it joined the stomach. The windpipe presented a normal appearance superiorly, but from about the middle of the trachea down to its smallest sub-divisions in the lungs it was much congested.

Chest.—The contained organs (with the exception of the congestion of the trachea) were healthy in structure. The right cavities of the heart were distended with dark fluid blood, the left were contracted and empty.

Abdomen.—The stomach was carefully tied at either extremity and removed. On being opened it was found to contain about half an ounce of a clear very viscid mucus, possessing no particular odour. The whole internal surface of the stomach was of a bright cherry red colour, which depended on the effusion of blood into the mucous and sub-mucous coats. There was no ulceration, and the peritoneal surface presented a normal appearance. The smaller intestines were much corrugated and congested on their external surface. About 4 feet of the upper portion of the small intestines were removed and opened. The mucous membrane was generally of a light pinkish hue, and was covered with a grayish-yellow tenacious mucus. The large intestines appeared healthy, and were contracted and empty. On being opened their mucous surface presented appearances similar to those noted in the small intestines. No fæculent matter was found in the body. The rest of the abdominal organs appeared to be healthy. The bladder was contracted and empty.

From the foregoing examination, we are of opinion that death in this case was caused by the action of some irritant substance on the stomach and bowels. The stomach and its contents were placed in a clean glass vessel. The duodenum and a portion of the small intestine, measuring fully 4 feet, were placed along with their contents in a clean jelly can. A portion of the brain, the right lung, the heart, a portion of the liver, the right kidney, the spleen, and the rectum were placed in a clean earthenware jar. About 3 ounces of blood, which had escaped from the heart and from the vessels of the brain, were collected and placed in a clean jelly can.

All these vessels and their contents were carefully removed by us to the Police Chambers. They were there labelled and sealed by us, and left in the custody of Dr Littlejohn till the following day (Sunday), when they were taken in our presence to Dr Maclagan's laboratory at Surgeons' Hall, and there delivered over to him and to Dr Littlejohn.

(Sigd.) Henry D. Littlejohn, M.D.
F. D. M'Cowan, M.D.

As is advisable in all cases of suspected poisoning, the stomach was *first of all* carefully removed from the body. The advantage of this procedure is that this important viscus by its removal is protected from the risk of being opened into when the other abdominal viscera are being examined. A portion of the contents

would thus certainly be lost. Again, if the removal of the stomach is delayed to a later stage of the dissection, it has necessarily by that time been subjected to so much displacement as to force a portion of the contents into the gullet, and this portion would be very apt to be allowed to escape unnoticed into the cavity of the abdomen. The heart was, without being opened into, examined *in situ;* and then the brain was inspected, in the hope that here a satisfactory explanation might be found of the symptoms of deceased,—the pain of the head and the urgent sickness and vomiting. Nothing was discovered, and the stomach was laid open along its smaller curvature. I shall never forget the striking appearance of vivid congestion which it presented as contrasted with the generally normal condition of the rest of the body. The accompanying sketch gives a pretty accurate delineation of the appearance of the inner surface of the stomach. There could be no doubt now as to the cause of this child's death. It had died from poison, and I felt fully justified in instructing the officers to take the mother into custody; and, as mentioned in the report, in addition to the stomach and contents, various portions of the body, together with a specimen of the blood, were removed for the purpose of chemical analysis.

The analysis was intrusted to Dr Maclagan and myself, and I confess that looking to the unmistakable evidence of irritation of the stomach, I thought that no difficulty would be experienced in detecting the special poison. One suspicious feature in this case was, that when the officer of police took possession—so to speak—of the house, it was ascertained that all traces of vomited matter had been removed by washing from the floor of the apartment where the child died, and also that several articles of clothing which it had on during its illness, and upon which it was possible it had vomited, had been put into a tub full of water.

The post-mortem examination, as has been stated, took place on a Saturday forenoon. The circumstances were duly reported to the public prosecutor, who in his turn applied to Crown counsel for authority to have a chemical analysis of the various productions in the case. It was late that day before Dr Maclagan could be communicated with, and I was requested to inform him that as it was of great importance that the nature of the poisonous agent employed should be ascertained by Monday morning, when the accused would be fully charged with the crime, it would be necessary for the analysis to be proceeded with on the Sunday. This was our excuse for what is stated in the report, that the various articles for analysis were handed over to Dr Maclagan on that day. According to our Scotch criminal procedure, every prisoner arrested on suspicion of a serious crime must be brought before the inferior criminal court and remitted to a higher, to allow of more extended investigation. A definite charge, however, must be preferred, to which the accused is asked, in presence of a magistrate, to plead, after which he is subjected by the public

Poisoning with Nitrate of Potash.

prosecutor to questions which must receive the approval of the magistrate, who sees that no undue advantage is taken of the accused. It is advisable to have a prisoner subjected to this ordeal as seldom as possible, hence it is important, in order to avoid a second inquiry, to have the crime stated as distinctly as possible. Thus to charge a person with having poisoned another without specifying the special poison is regarded as somewhat vague, and as involving some hardship to the accused, and it is always considered necessary, if possible, for the prosecutor to be able to name the substance which has been used.

Due search was, therefore, made for the more ordinary metallic irritant poisons, but without success,—neither arsenic, mercury, antimony, lead, zinc, nor oxalic acid were present. We agreed to continue the search on the following day. As I was returning home late that evening I was accosted in the public street by the detective officer who had charge of the case. He mentioned that at the back of a cupboard he had found a pair of child's boots which had been worn by the deceased, and that on the toe-part of one of the boots there was some appearance of minute crystals. The boots were produced from under his coat, and by means of the rather uncertain light from a street lamp I made out, but with difficulty, indications of a scanty crystalline deposit. This, were it due to matters administered to or vomited by the child, at once threw light on the nature of the poisonous agent. It was crystalline; with a lens the crystals were determined to be prismatic and fluted. Dr Maclagan, when he saw them next morning, pronounced them to be nitre, and on a very small quantity being removed and dissolved in distilled water, the solution gave, with brucia, the usual vivid red reaction. No time was lost in completing the analysis, and as the result nitre was detected in the mucus lining the stomach and intestines, in the liver, lung, kidney, brain, and in the blood. From the stomach it was separated in the crystalline form; in the other organs the reactions with brucia and iron were well marked. In the brain and blood the iron test was not decisive, but the brucia test showed the presence of nitre very distinctly.

All the articles of clothing recovered in the house were carefully examined; the strings of a bonnet worn by the deceased, which were damp and had a sour smell, were more or less stained at various parts; these stains contained nitre. In the folds of the left string were found a few crystalline particles, which proved to be fragments of crystals of nitre. These evidently had not been deposited like those on the boot by crystallizing from a fluid saturated with nitre. They were not fine crystals, but broken fragments, and they did not adhere to the surface like the crystals on the boot, but lay loose in the folds of the string and could be shaken off. Two pieces of cotton cloth were recovered, which were suspected to have been used to wipe up the discharges from the bowels. They bore stains apparently due to watery fæcal discharges,

and afforded distinct indications of the presence of nitre. We concluded our report by stating—"That from our whole experiments we can come to no other conclusion than that this child must have swallowed a very large dose of nitre, which had not only impregnated the blood and all the organs, but had been discharged in considerable quantity in the matters vomited and passed by stool."

That this child had died of poisoning there could be no doubt, and the woman was fully committed for trial on the capital charge. The evidence adduced showed that the child, apparently in good health, was taken out by his mother at 5.35 P.M., and at 6 o'clock, when she returned, the child was cold, sick, and vomiting. The accused remarked that it had vomited on the street. The bowels now acted, and before the child could be somewhat warmed and put to bed, it purged four times and repeatedly vomited. The landlady, seeing the serious state of the child, suggested that a doctor should be sent for, but the prisoner refused to go for one, and when it was suggested that assistance might be obtained from the neighbours, she peremptorily declined to allow them to be called. The child meanwhile remained quiet in bed, and was only heard to cough twice. Shortly after the landlady went to look at him, and found him dead. This was about 8 o'clock. On one point the evidence for the Crown was deficient, and that was proof of the purchase and administration of the poison. Nitre differs from most active irritants in the facility of purchase. Sold as it is in every grocer's shop, its purchase attracts no attention, and it becomes impossible for the seller to identify the purchaser of such a common commodity. In the present case, the prisoner lived in a crowded neighbourhood, and on the tradesmen being interrogated as to their sales on the day in question, several remembered selling quantities of nitre, but when the woman was placed among other prisoners they failed to recognise her. At last one grocer spoke distinctly to selling a small package of the substance to a female as he was engaged in transacting some business with a commercial traveller, and he identified a shawl which the accused had borrowed from a neighbour and had on that evening, but on allowing the woman to mix freely with other females the witness could not positively say she was the purchaser. As to the mode of administration, no one could be found who saw the woman walking with the child that afternoon. There are several public wells in the neighbourhood, and it was thought highly probable that she would avail herself of one of these, but of this there was no proof. One neighbour hinted, from some expression used by the accused, that an orange had been employed in giving the nitre to the child, but no orange pulp could be found in the body.

These deficiencies in the evidence for the Crown were strongly commented on by the defence, and it was urged on behalf of the accused that the poisoning might have been accidental. Such a common substance as nitre lying exposed in shops might be spilt on the floor, and thus be swept into the street. Should the child have

been left by its mother, its attention might have been attracted to the white deposit in the gutter. On tasting this, the child, pleased with its coolness, would be inclined to take more, and thus it was possible a poisonous quantity might be taken by a heedless child. Or again, the child, if left unprotected by its mother, might have the poison administered to it by some unknown party. Such lines of defence were highly improbable; but as important links in the chain of evidence were awanting, the jury returned a verdict of "not proven," affirming the strong suspicion that attached to the prisoner, and justifying the action of the Crown in bringing her to trial, but, at the same time, giving her the benefit of the doubts suggested by the evidence.

The case is on many accounts of interest, scientific and professional. It is believed to be the only case of criminal administration of nitre hitherto recorded. The taste and the rapidity of its action naturally are serious obstacles to the use of nitre by the poisoner, and, as the present case shows, children are the most likely victims. The wonder is that, with the unusual facilities for the purchase of such a very active poison, we do not hear of nitre being more frequently employed.

The Senior Vice-President, Dr David Wilson, who occupied the chair during the reading of this communication, stated that the President's paper was especially valuable because of the advice given with regard to professional conduct in delicate and difficult circumstances.

Dr Ireland never had the fortune or misfortune to meet with a case of poisoning by nitre. When he commenced his medical life he was interested in the treatment of rheumatism by nitrate of potash in large doses. He had tried the remedy when a student in his dispensary cases to the extent of half an ounce daily. The President had not told them at what dose the drug was poisonous. It had been given in Paris up to 60 or 70 grammes (about 3 ounces) daily, but well diluted. He remembered Valleix said he had seen two cases of sudden death where 12 grammes of nitrate of potash had been given. The symptoms were coldness, weakness of the extremities, and sinking.

Dr Craig said the President's paper was not one that left much room for discussion, because most of them would agree with the ethics of it. There was no doubt that poisoning by nitrate of potash would depend on the amount of concentration of the drug, as in the case of any other irritant. He always remembered a remark made by Prof. Maclagan when he was a student, that nitre was one of the few poisons for which they possessed no antidote. It was difficult to say what might be a fatal dose of it or any other irritant on account of the vomiting set up. If a sufficient quantity were vomited the patient's life might be saved. He thought, however, that from half to one ounce in a concentrated form would probably prove poisonous.

Professor Annandale rose to speak on the question of ethics in such cases. He wished to ask the President whether in a case of suspected slow poisoning it would not be better for a young practitioner to call in a skilled toxicologist as a consultant before informing the Procurator-Fiscal or other authorities.

Mr Caird drew attention to Sir Robert Christison's dictum that in such cases the practitioner's duty was to inform the patient only of his suspicions. He should like to know if Dr Littlejohn approved of this advice.

The President said he quite agreed with Professor Annandale, and in large towns it was probably the best and easiest course to have first of all a consultation. In country districts it was not so practicable. When he advised that the medical attendant should communicate his suspicions to the authorities, he referred more particularly to cases where death had occurred. Dr Bramwell had asked him an important question privately. What would he do in England? He confessed there was more difficulty in keeping any investigation private there; but he thought the proper thing would be to write to the coroner a letter, asking him to regard the communication as confidential. If the patient were still alive the difficulties were greater. Sir R. Christison's advice might be followed, but he thought it would be hazardous to do so in all cases. It might either prove too great a shock, or might make the patient attempt to shield the suspected person, and thus defeat the ends of justice.

2. THE EXTERNAL AUDITORY MEATUS IN THE CHILD.

By J. Symington, M.B., F.R.C.S. Ed., Lecturer on Anatomy, Edinburgh.

In this paper I propose to give the results obtained from a study of a series of frozen sections of the external auditory meatus in the child, and to compare them with the various views held with regard to its anatomy.

An examination of the literature on this subject will show that this method of investigation has been almost entirely neglected. The only illustrations of frozen sections that I have succeeded in finding are a few somewhat diagrammatic representations of sections of the meatus in the new-born infant.

For facility of comparison most of my sections were made in the same direction, viz., transverse vertical, or coronal. The meatus was generally divided nearer the anterior wall than the posterior, and in all of them the posterior part of the section was traced, so that the specimens in the Plate are represented as seen from the front. I have also made a few sagittal and horizontal sections. All the drawings of sections are life size, and made from tracings taken while the parts were frozen. To check and supplement the results obtained from the frozen sections, a number of ears were dissected in the ordinary way, and the skulls of several children were macerated.

Length of the Meatus.—In estimating the length of the meatus no difficulty is experienced in defining its inner extremity, for this is clearly marked by the membrana tympani; but as this membrane passes from above downwards and inwards, and from behind forwards and inwards, none of the walls extend to within the same distance from the mesial plane of the body. With regard to its external boundary, this is not so well defined; and, according to Gruber,[1] the very various estimates of the length of the meatus are mainly to be attributed to a want of agreement as to its limits externally. Some anatomists, Meckel for example, have even included the tragus in their measurements of the walls of the meatus. The only part of the outer end of the meatus which can naturally be distinguished from the concha of the pinna is its posterior wall, for this turns abruptly backwards, while the other walls pass outwards into the pinna without any distinct line of demarcation. The majority of authorities now agree with Von Tröltsch in taking as the external boundary of the meatus a sagittal plane passing through the outer end of its posterior wall. The most careful and exact measurements of the length of the meatus are those of Von Tröltsch. He gives 24 mm. as its average length in the adult, its anterior wall being 27 mm., lower 26, posterior 22, and upper 21. I have not been able to find any precise statements as to its length in the child; and, indeed, in almost all works on the anatomy of the ear there are no allusions to this point. The subject is, however, referred to in Quain's *Anatomy*, 9th edition, vol. xi. page 434, as follows:—"The auditory passage is very short and rudimentary in the infant, for the osseous part begins to grow out of the temporal bone only after birth, and thus the internal and middle ears are much nearer to the surface than in the adult."

As the walls of the meatus in the infant are soft and yielding, it is obvious that care must be exercised in measuring its length not to stretch or compress it any way. The length of other passages—for instance, the vagina—has been greatly exaggerated by not attending to this rule. To avoid such sources of fallacy I have estimated its length from frozen sections, and on next page will be found a table of a series of measurements of the roof and floor of the meatus obtained by this method.

If we compare these measurements with the length in the adult, as given by Von Tröltsch, it will be evident that the meatus, instead of being shorter, is relatively longer than in the adult. Thus, in the new-born child the floor is 20 mm., in the adult it is 26 mm. or little more than one-fourth longer. As one would naturally expect from this, the meatus increases very slowly in length after birth; indeed, from my cases, it would appear at first to diminish. My specimens are too few to warrant any definite conclusions; but

[1] *Lehrbuch der Ohrenheilkunde mit besonderer Rücksicht auf Anatomie und Physiologie*, Wien, 1870.

I am disposed to think that the opening up of the meatus, which occurs after birth, is accompanied by some slight diminution in its length. Henle states that the estimates of the length of the meatus vary from 22 mm. to 42 mm. These discrepancies have arisen from a want of agreement as to its external boundary (Gruber), and also probably from the method of examination. My results show only very slight individual variations.

No.	Age.	Length of Floor.	Length of Roof.
1	Fœtus, 7 months old, . .	16 mm.	10 mm.
2	" 9 " . .	20 "	15 "
3	" 9 " . .	20 "	15 "
4	" 9 " . .	19 "	15 "
5	Child, 2 " . .	17 "	13 "
6	" 6 " . .	19 "	14 "
7	" 12 " . .	20 "	15 "
8	" 2 years old, . .	22 "	16 "
9	" 5 " . .	23 "	16 "
10	" 6 " . .	24 "	17 "

The practical importance of an accurate knowledge of the length of the meatus needs scarcely to be insisted upon; but these measurements are also of interest in connexion with the relative size in the child and adult of other parts of the ear. It is well known that the middle and internal ears are nearly as large at birth as in the adult, and the same would appear to be the case with the external ear, so far as the length of the meatus is concerned.

Lumen of the Meatus.—In the fœtus the meatus is closed, its lower wall being in contact internally with the membrana tympani, and external to it with the superior wall. Towards the end of fœtal life these become separated by an accumulation of epithelium or vernix caseosa in the meatus. This is first found at the inner end of the meatus, between the floor and the membrana tympani. The coronal section of the meatus of a nine months' fœtus, seen in the Plate, Fig. 1, shows only slight separation of the walls, but in another subject at the same age the meatus was rather more opened up. In both cases respiration had not occurred.

At birth the walls of the meatus are either in contact or the cavity is filled with vernix caseosa, so that a child is born with its ears practically closed.

According to Kussmaul,[1] the most discordant sounds made near a new-born child, when awake, do not apparently disturb it. I am not aware of any observations as to how soon after birth, or by what agencies, the meatus becomes a distinct air passage. In an infant six days old I found the walls of the meatus more separated than is normally the case with a new-

[1] *Untersuchungen über das Seelenleben der neugebornen Menschen*, Leipsig, 1859.

born child, but its cavity was nearly full of detached epithelium, etc. In a child, aged two months (see Plate, Fig. 2), the meatus was patent and contained air. The lumen of the meatus would appear to gradually increase in size after birth, but so slowly that it is relatively narrow in the infant as compared with its length.

By the growth of the tympanic bone the osseous meatus is usually formed between the ages of twelve and fifteen months. From an examination of several specimens about this age, kindly lent me by Professor Turner, and from others in my own possession, I found that the osseous meatus measured from above downwards and backwards on an average 6 to 7 mm. In the adult it is generally from 8 to 9 mm.

Walls of the Meatus.—Most of the standard works on anatomy give a very imperfect description of the structure of the walls of the meatus in the child. They state that in the adult the meatus consists of an osseous and a cartilaginous portion, while in the new-born infant there is no osseous meatus; and the reader is too often led to infer that at that period the meatus is entirely cartilaginous. Indeed, Tillaux in his *Traité d'Anatomie Topographique*, 3rd edition, 1882, page 99, says:—"Chez l'enfant nouveau-né le conduit auditif est donc exclusivement fibro-cartilagineux." It was shown, however, more than thirty years ago by Von Tröltsch that this idea is erroneous, for, in the new-born child, as in the adult, the outer part only is cartilaginous, and in the new-born infant the future osseous portion is represented by fibrous tissue into which ossification extends to form the greater part of the walls of the osseous meatus. This membrane, which might be called the fibrous or membranous tympanic plate, is shown in section in the floor of the meatus in the Plate, Figs. 1, 2, 3, and 4. At birth it is thin; but it becomes thicker and firmer in a few months. Fig. 6, a sagittal section of the meatus in a child four months old, made 9 mm., external to the inner end of the floor of the meatus, exhibits its relation to the anterior and inferior walls of the meatus. Julius Böke[1] maintains that this structure, described as membrane by Tröltsch, really consists of cartilage composed of fibres with scattered cells. In the account of the minute anatomy of the external ear by J. Kessel in Stricker's *Histology*, there is no reference to it. I have satisfied myself by microscopic examination that it is compact fibrous tissue and not cartilage.

It is generally taught that at birth there is no osseous meatus. This scarcely conveys a correct idea of the condition of walls of the meatus at this period, for there is then an osseous roof and posterior wall. The roof is shown in the coronal section of the meatus of the new-born child in the Plate, Fig. 1, and its gradual increase is seen in Figs. 2 to 5. The sagittal section in

[1] *Virchow's Archiv*, Bd. xxix., 1864.

Fig. 6, from a child aged 4 months, was made just internal to the cartilaginous portion. It shows the bony posterior and superior walls, and the anterior and inferior fibrous walls.

The formation of the tympanic portion of the osseous meatus has been investigated by Humphry, Von Tröltsch, Bürkner, and Zuckerkandl. The latter is quoted by Politzer[1] as describing the formation of the tympanic bone as commencing by the rapid growth of the anterior and posterior tubercles found on the tympanic ring of the new-born child. The lower part of the tympanic ring grows out more slowly, and the two tubercles uniting, a foramen is left in the tympanic bone which sometimes remains throughout life. An essentially similar description was given by Humphry in his *Treatise on the Human Skeleton* published in 1858, and in plate xvi. of the same work there are several drawings illustrating this process. After the union of the two processes of the tympanic bone, a distinct osseous external auditory meatus exists. Von Tröltsch gives a drawing of a temporal bone of a child about three years old, in which a gap open to the outer side is shown in the tympanic bone. The rate of ossification of the tympanic bones varies somewhat, but as a rule the tubercles of the tympanic ring unite at or soon after the end of the first year.

Explanation of Plate.

Fig. 1. Coronal section of right ear of a nine months' fœtus.
Fig. 2. Coronal section of left ear of a child two months old.
Fig. 3. Coronal section of right ear of a child six months old.
Fig. 4. Coronal section of left ear of a child twelve months old.
Fig. 5. Coronal section of left ear of a child five years old.
In Figs. 1 to 5: *a*, tympanic bone; *b*, fibrous tympanic plate; *c*, fibro-cartilage.
Fig. 6. Sagittal section of left ear of a child four months old; *t*, tympanic cavity; *m*, external auditory meatus; *b*, fibrous tympanic plate; *i*, inferior maxilla.

Dr M'Bride stated that the paper just read was so full of carefully collected and demonstrated facts that it admitted of but little discussion. He felt sure that both anatomists and aurists could not help being grateful to an observer so careful as Dr Symington for taking up a subject which was so ill understood, and about which so many conflicting views existed. There were, however, two points which he would like to refer to. In the first place, he would like to ask Mr Symington how he accounted for the rapid change in the position of the tympanic membrane which must, according to his diagrams, occur between the seventh and ninth months. In the second place, it was stated by Mr Symington that the development of bone in the formation of the osseous portion of the meatus began below and in front. In this statement Mr Symington directly opposed the views of Professor

[1] *Lehrbuch der Ohrenheilkunde*, 1878.

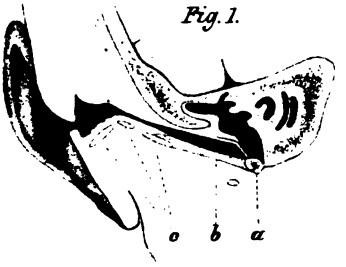
Fig. 1.
c b a

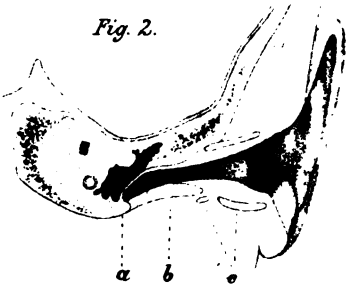
Fig. 2.
a b c

Fig. 3.
c b a

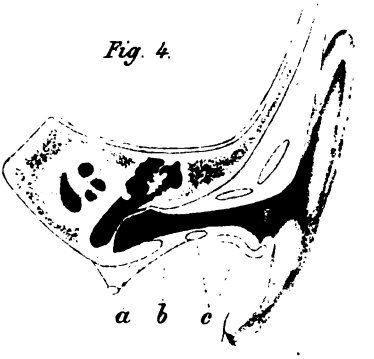
Fig. 4.
a b c

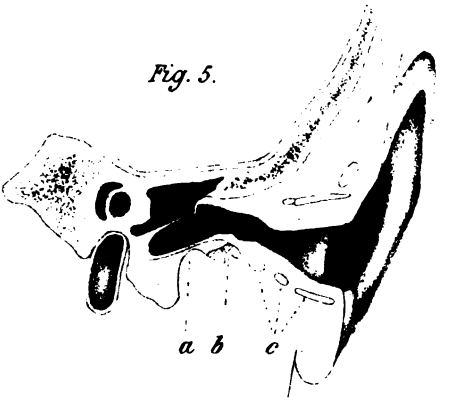
Fig. 5.
a b c

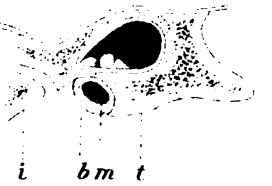
Fig. 6.
i b m t

Zuckerkandl of Gratz, who was looked upon on the Continent as one of the first authorities on the anatomy of the nose and ear. He had shown that ossification occurred by the further development of two tubercles situated on the tympanic ring. These tubercles finally coalesced, leaving sometimes a permanent foramen in the anterior inferior wall—the "ossificationslücke" of Von Tröltsch. This opening is commonly found up to the second or third year. Dr M'Bride, from an examination of specimens shown him by Professor Zuckerkandl—with whom he had made many dissections of the ear—had little doubt of the correctness of these views, which pointed to a regular, definite, and constant process in the ossification of the external auditory meatus.

Dr Kirk Duncanson said he had great pleasure in hearing Dr Symington's valuable paper, but thought it was deficient in that there had not as yet been enough dissection. Although much superior in its method of investigation to former methods, and to what he (Dr Duncanson) had himself done, still there might be faults found, as, for example, the membrana tympani in the six months' fœtus diagram was shown not attached to the annulus tympanicus at all, but apparently to the inner wall of the tympanum. That might be due to the action of the saw carrying these soft delicate structures before it, and the same cause might apparently lengthen the external auditory meatus. He certainly, in many dissections of the ear in the fœtus of different ages, had never but found the membrana tympani attached to the annulus tympanicus as in the adult, except at its upper portion where the bone is wanting. He had always found the apparently horizontal position of the membrana tympani in the fœtus as shown in the diagram. Was the more perpendicular position in the child and the adult not brought about by the groove carrying the membrane in the annulus tympanicus being carried outwards as ossification proceeded in the fibrous or fibro-cartilaginous plate in the floor and walls of the meatus? Or was this assisted by an increase in the bony parts forming the base of the skull? Mr Symington's measurements ought to be continued, taking them from the fixed points, both of the outer and inner extremities of the meatus in fœtal and adult skulls. He thought, however, that some deformity must take place in the freezing and section-cutting of frozen parts, as shown by the diagram representing the normal adult ear, which could not be correct.

Mr Symington said that the ossification which extended outwards from the tympanic ring to form the tympanic plate formed the anterior and inferior wall of the osseus meatus. He had not completed his investigations with regard to the inclination of the membrana tympani. With reference to Dr Kirk Duncanson's criticisms of the diagram, he had to state that both the section and the drawing he had exhibited showed the membrana tympani attached to the annulus tympanicus; but the artist he had employed to make the large diagram had made the mistake of separating them.

Meeting III.—January 7, 1885.

Dr H. D. Littlejohn, *President, in the Chair.*

I. Election of New Members.

The following gentlemen were elected Ordinary Members of the Society:—William Wilson, M.B., L.R.C.S. Ed., Edinburgh; John Mowat, M.D., Edinburgh; Skene Keith, M.B., C.M., F.R.C.S. Ed., Edinburgh; D. Nöel Paton, M.B., B.Sc., Edinburgh; George Hugh Mackay, M.B., C.M., Elgin; J. Michael Dewar, M.B., C.M., Edinburgh.

II. Exhibition of Patients.

1. *Mr Joseph Bell* showed a case of EXCISION OF THE ELBOW-JOINT FOR INJURY. The patient, a young man, æt. 19, got his right arm lacerated by a circular saw. The injuries were very severe, the skin on the back of the elbow being destroyed to the extent of nearly 6 inches, the bones exposed, and the joint opened into. Amputation was at first thought to be necessary, but an attempt was made to save the limb. He was at first treated in the country by Dr Kynoch of Greenlaw, and not sent in to hospital till the wound had healed. The joint had then anchylosed. There was neither pronation nor supination, and the arm was perfectly straight. The cicatrix was thin and feeble. The lad being anxious to have more use of his arm, submitted to the operation of excision of the joint after its risks had been explained to him. The operation was a little tedious, the periosteum having to be carefully separated before the saw was applied. Thirty-five days had elapsed since its performance, but the patient had already very remarkable movement and power in the limb, perfect flexion and full voluntary extension, with both pronation and supination. An examination of the specimen showed that osseous anchylosis had occurred.

2. *Dr Duncan* showed a patient whose case illustrated the good effects of the SUBCUTANEOUS LIGATURE OF VARICOSE VEINS. His internal saphena vein had been much enlarged, and there were two huge ulcers of the leg depending on its varicose state. The ligatures were applied in six different places along the course of the vein. The patient lay five weeks in the Infirmary, during which time his ulcers healed, and the cure of the varicosity of that vein was thorough. The method of operating was to pass a needle through the skin beneath the vein, and bring it out on the other side. The needle was then threaded with catgut and withdrawn, and the catgut left in its track. It was then passed from the first point of insertion between the skin and the vein to the

point of exit and unthreaded. The loop of catgut round the vein was then tied. The limb was dressed with salicylic wool, and not touched again till healed.

3. *Dr M'Bride*, through the kindness of Professor Chiene and Dr Hamilton Wylie, was enabled to show a boy, æt. 8, who was affected with OBJECTIVE TINNITUS, or, in other words, a CEPHALIC MURMUR. It was heard loudest over the left temporal bone, was continuous, while at each systole of the heart it rose into a whistling note, and had thus the characteristics of the murmur of arterio-venous aneurism. From its being remote from the auscultating ear, he considered it due to some vascular change within the cranium, and probably to an arterio-venous aneurism between the carotid and the cavernous sinus. The report of the ophthalmic surgeon that there was congestion of the left optic disk, and not of the right, strengthened this view. The history of the case was that some weeks previously the boy had fallen on his head, become insensible, and shown symptoms pointing to fracture of the base of the skull. On his return to consciousness this murmur was present, and was both objective and subjective. Dr M'Bride fancied that it was difficult to come to any other conclusion than that it was due to arterio-venous aneurism. It was always stopped when the common carotid was compressed.

III. Exhibition of Pathological Specimens.

1. *Dr Craig* showed a specimen of SARCOMA OF THE RIBS, LIVER, AND LUNGS. The disease had affected the fifth, sixth, and seventh ribs of the right side. It grew first on the sixth rib, and the patient, a man, æt. 44, lived about six months after Dr Craig's attention was first drawn to it. It was then not much larger than a small bean. He showed it partly on account of the difficulty of diagnosis, because although seen by several distinguished members of the profession, the opinions as to its nature varied considerably. His first suspicions of malignancy were aroused by the fact of the man's losing flesh rapidly without anything else to account for it. The tumour was about the size of a small orange, and projected not quite so far into pleural cavity as outwards. It was not adherent to the pleura. Secondary deposits occurred throughout the liver, and the lungs also showed traces of being affected. The patient had been strong and healthy, though rather addicted to alcohol, before the tumour appeared. There had been no injury so far as was known, and no instance of malignant disease in the family history.

2. *Dr James Carmichael* exhibited the BRAIN of a patient who died in the Royal Hospital for Sick Children four days after admission. The girl, aged 8 years, had suffered six months pre-

viously from a discharge from the right ear, which ceased at the end of a fortnight. She remained well until three weeks before admission, when she became ill, and the otorrhœa again made its appearance. Dr Cadell saw the child and evacuated an abscess over the mastoid process behind the ear. Serious symptoms having supervened, the child was sent into Hospital. When admitted she was delirious, and constantly talking. On examination of her ear, a very fœtid discharge was escaping from a wound behind, and also from the meatus. There was inequality of the pupils, the right one being dilated and irresponsive. The pulse was 56, the temperature 99°; no paralysis or spasm of any muscles. The child continued much in this state for two days, and then became comatose. Mr Annandale kindly saw her with me, and it was agreed, as the child was in a hopeless state, to give her a possible chance of relief by opening the skull over the affected part, in case there should be any accumulation of pus within the cranial cavity. The mastoid process of the temporal bone being carious, the bone was gouged away until the membranes could be felt. During the operation some pus escaped, but in pushing a trocar and cannula through the dura mater there was no further escape of matter. The child was then put to bed, and seemed to regain sensibility to a slight extent, so as to be able to swallow a little food. She, however, soon relapsed into a helplessly comatose condition, and died the following day. On post-mortem examination, when the skull cap was removed, about an ounce and a half of pus escaped from between the bone and the dura mater. Between the brain itself and membranes a considerable quantity of serous fluid was found, but no pus. A rugged opening in the dura mater was found over the petrous portion of the temporal bone, opposite which, when the brain was removed, a small opening was discovered in the cerebral substance over the lower and anterior part of the temporo-sphenoidal lobe. From this opening serous-looking fluid was escaping, apparently from the lateral ventricle on this side. The cerebral substance over this portion of the brain was sodden and gangrenous looking. On enlarging the aperture it was found that it led into a gangrenous abscess cavity about the size of 2 inches by 1¼. The brain's tissue between this and the lateral ventricle was soft, greenish, and gangrenous looking. The case was one by no means uncommon, and showed the ultimate danger in cases of otitis in children, and the need of early care and treatment in affections of this sort.

3. *Dr MacGillivray* showed for Professor Chiene a TUMOUR INVOLVING PART OF THE ŒSOPHAGUS, PHARYNX, AND LARYNX, removed from the patient shown at the previous meeting of the Society. Œsophagostomy had been performed and the patient sent home, to see if she gained flesh before the removal of the tumour was

attempted. She returned to hospital in a few days on account of difficulty of breathing which had occurred. Mr Chiene proposed tracheotomy, but she refused to submit to anything, unless an attempt was also made to remove the disease. The dangers were explained to her, but she determined to take the risk. Tracheotomy was first performed below the cricoid cartilage. The incision for the œsophagostomy was reopened and extended upwards, the sterno-mastoid and vessels turned aside, the œsophagus cut across, and the lower end stitched to the skin. The upper portion was then separated from the trachea and the bodies of the vertebræ, along with the posterior and lateral parts of the pharynx, which were involved. The box of the larynx was then found to be affected, and was removed by an incision carried along the hyoid bone from the top of the incision made for the removal of the œsophagus. The tracheal wound was then stitched to the skin. The condition of the patient was not quite satisfactory since the operation, a pneumonia having occurred, probably from some discharge having got into the trachea, and passed down into the lung.

4. *Dr Kirk Duncanson* showed (*a.*) A SARCOMA OF THE HARD PALATE removed after the parts had been three times within ten minutes well painted over with a 20 per cent. solution of hydrochlorate of cocaine. The patient felt no pain until at the end of the operation, when, as the root of the tumour was being cut out, he experienced pain. Dr Woodhead reported the tumour to be a round-celled sarcoma, with myxomatous degeneration. (*b.*) Specimens of OTO-MYCOSIS, the fungus disease of the ear; cultivations and microscopic preparations, also micrococci from a case of furuncle of the external auditory meatus.

5. *The President* showed (*a.*) a STOMACH presenting two perforating ulcers, evidently of some standing. These ulcers were nearly opposite each other at the middle of the viscus. The interest of the case lay in the absence of symptoms of such a serious disease and in the rapidity of the death. There was no history of stomach disease. Deceased was of intemperate habits, and the day before her death was going about her usual household duties. She suddenly complained of pain in the abdomen, and rapidly sank with the symptoms of general peritonitis. Behind the stomach, but not attached to it, was a circular tumour the size of a hen's egg, which, on section, showed a structure resembling encephaloid. (*b.*) A PORTION OF THE ILEUM from a typhoid patient. There were two perforating ulcers in close proximity. The patient was in the City Hospital from 18th to 28th December, and was progressing favourably, when a sudden relapse occurred, and death took place in three hours.

IV. Original Communications.

1. ON SOME INJURIES IMPLICATING THE ELBOW-JOINT.

By Thomas Annandale, F.R.C.S. Ed., F.R.S.E., Regius Professor of Clinical Surgery in the University of Edinburgh.

Having had an opportunity of dissecting and studying several illustrative specimens of some of the rarer forms of injury involving the bones connected with the elbow-joint, I desire to give a description of them, with the hope that my observations may possibly add to our knowledge of the pathology and treatment of these injuries.

The specimens which I propose to describe are twelve in number. Nine of them are from my own collection, and eight of them have been obtained by the operation of excision, the operation being performed to relieve anchylosis or deformity, the result of an injury to the joint. In all these eight cases the injury had been "simple" in its nature. Two others belonged to the collection of the late Professor Spence; and I am indebted to my colleague Professor Chiene for an opportunity of examining and describing them. One I obtained from the University dissecting-room, and the last one is preserved in the Anatomical Museum of the University.

It may be here noted that in addition to displacements or fractures, or both, of the bones as the immediate result of the injury, all these specimens illustrate other and secondary changes, which are surgically important in connexion with diagnosis and treatment. These secondary changes may be enumerated as ossifications, atrophies, and adhesions, fibrous or osseous.

For convenience I propose to divide these cases into three groups.

I. Fracture of Coronoid Process.

Case I.—A boy, about 12 years of age, injured his elbow by a fall, the result being a stiff joint and useless arm. The joint was excised, and the dissection of the parts removed showed a dislocation of both bones of the forearm backwards. The tip of the coronoid process was broken off, drawn inwards and upwards, and attached by fibrous adhesions to the posterior aspect of the internal condyle of the humerus. The head of the radius had formed a shallow groove on the posterior part of the external condyle, and rested upon this point, to which it was attached by some fibrous adhesions and a small quantity of osseous matter.

Case II.—A boy, about 18 years of age, injured his elbow by a fall, with a similar result as in the former case. Excision of the elbow-joint afforded an opportunity for an examination of the parts, which presented the following appearances:—Both bones of the forearm were dislocated backwards, and the coronoid process had

been fractured through its base. A large quantity of osseous material had been developed in the region of the fracture, and by means of it this process was united to the ulnar and also to the posterior aspect of the condyles of the humerus, close to the olecranon fossa.

CASE III.—An adult, suffering from anchylosis and deformity of his elbow, the result of an injury, had his elbow-joint excised. In this case there was partial dislocation outwards of the bones of the forearm. The tip of the olecranon rested on and was attached by osseous union to the outer margin of the olecranon fossa; the head of the radius projected external to the external condyle, its ulnar margin resting on the outer edge of the condyle. The coronoid process had been separated, and it was united to the ulna and condyles of the humerus by means of a considerable amount of osseous material.

In these three cases the fracture of the coronoid process is associated with other injuries. The first two cases are, I think, illustrative examples of a fracture of this process occurring in connexion with a dislocation backwards of both bones of the forearm.

It is generally acknowledged by surgeons that fracture of this process is rare, but there is still a difference of opinion as to its frequency, and also as to the frequency with which it is associated with backward dislocation of the ulna. Spence says, "I have never seen a case of that dislocation in which the coronoid process was broken." Hamilton only gives one well-authenticated example of this accident with backward dislocation. Malgaigne thinks this fracture more common than is usually supposed. Ferguson writes that in dislocations of the bones of the forearm backwards the coronoid process will "probably" be broken. Hulke records a case in which both coronoid processes were fractured with dislocation backwards of the bones of both forearms. Sayre reports a case of this dislocation, in which the coronoid process was separated and was adherent to the anterior aspect of the condyles of the humerus. Holmes refers to a similar case in which both coronoid processes were fractured and adherent to the humerus.

From the fact that two cases of this fracture with dislocation backwards of the bones of the forearm have been verified by dissection from cases occurring in my own practice in the course of a few years, I am inclined to express the opinion advanced by Malgaigne that the accident as a complication of this dislocation is not so uncommon as has usually been supposed.

It is also probable that this fracture may not always be recognised in cases of dislocation, and as a dislocated elbow-joint is, after the reduction of the bones, usually kept in the flexed position for some time, the coronoid process when fractured will also be kept in position, and so be likely to unite.

The third case illustrates a lateral dislocation of the bones of the

forearm with fracture of the coronoid process, and it is an injury which I am not aware has been previously described.

Uncomplicated fracture of the coronoid is probably the rarest form of fracture of this process. Packard, in Ashhurst's *Encyclopædia of Surgery*, divides fractures of the coronoid into—(1.) Those in which the tip only is broken off; (2.) Those in which the whole process is separated; and he also refers to those complicated by fracture of neighbouring bones. The first of these is, I should think, always associated with dislocation of the ulna backwards, or with other injuries, and the second may also be associated with this dislocation or other injury, or may occasionally be the result of muscular action alone.

II. Fractures of the Head or Neck of the Radius Uncomplicated.

Case IV.—A man, æt. 48, was recently admitted into my wards with the following history:—Three months before admission he was lifting stones by means of a shovel, and received a severe jerk to his right elbow by striking the wrist of the corresponding arm against his knee. The accident caused pain over the head of the radius, and considerable swelling of the joint soon followed. The patient did not apply for medical advice, thinking that he had simply sprained the joint. Six weeks after he attended the out-patient department, when a fracture of the upper end of the radius was diagnosed, as there was distinct crepitation and mobility felt in this situation, and the head of the bone rotated only to a slight degree when pronation and supination of the forearm were practised. The olecranon was displaced slightly inwards, and rested upon the inner margin of the olecranon fossa, and the forearm was in the position of semi-pronation. Flexion and extension of the joint were not quite complete, and perfect pronation and supination could not be performed. An attempt was made to place the joint in the semi-flexed position with the forearm in the extreme supine state, but the latter could not be perfectly accomplished. A splint was applied to retain the limb as nearly as possible in this state, and after two weeks of this treatment it was found possible to thoroughly supinate the arm, and accordingly it was secured in this position by means of a splint. At the end of a month no union had taken place, and I accordingly cut down upon the head of the radius with antiseptic precautions, and then I found that the head of the bone was lying loose, having been separated entirely through the neck. There was no attempt at union, and atrophy of the neck had taken place to a considerable extent. Having removed the separated head, the wound was closed and the arm placed in the supine position, with the elbow semi-flexed. I could detect no other injury except the displacement of the ulna already referred to. After the operation the

movements of the joint were improved, but the patient still remains under observation, the wound not yet having healed.

CASE V.—A young woman fell and injured her elbow nine months previous to being seen. The elbow-joint was, on examination, found to be firmly anchylosed in the extended position. Excision of the joint was performed, and the removed parts illustrated an oblique fracture through the head of the radius, which separated the articular surface of the bone to the depth of about a quarter of an inch into two halves; the anterior half remained in position attached to the neck of the bone; the posterior half was displaced upwards and backwards, and was attached by firm fibrous adhesions to the posterior surface of the outer condyle of the humerus.

CASE VI.—From the late Professor Spence's collection. The right arm of an adult with the elbow-joint laid open, and showing an oblique split through the articular surface of the head of the radius. This split completely separates the articular surface to the depth of a quarter of an inch into two halves, one-half remaining attached to the neck. There is only very slight separation of the fragments. The accident was of recent origin, and no secondary changes could be observed.

CASE VII.—This is also from the same collection. It was the elbow of an adult, with an exactly similar injury, but it is of old standing, and the fragments are united by fibrous adhesions.

Case IV. must be considered as one of great interest and rarity, and one which has been verified by opening the joint. It is acknowledged by all surgeons that uncomplicated fracture of the neck of the radius is of extreme rarity, and, in fact, some are of opinion that it cannot occur. Hamilton says that such an injury is "extremely rare." Spence writes, "The highest fracture of the radius is that which occurs a little below the insertion of the biceps." Holmes states, "Fracture of the upper part (head or neck) of the radius is another of the proved complications of dislocation, and is believed by some authors to occur independently, but without anatomical proof." Packard remarks in regard to fracture of the neck of the radius, "But no instance is known to me in which it has been ascertained to be broken by itself."

Further, this case is interesting from the fact that the injury was not caused by any direct force, and it also illustrates the non-union and atrophy of the neck of the radius.

Cases V., VI., and VII. are all well-marked examples of oblique or longitudinal fractures through the head of the radius uncomplicated by other injury, and they must also be classed as rare specimens. No mention is made of the last two in Spence's valuable work on surgery, but the condition seen in Case VII.

shows that fair union is possible after such an injury, provided no great displacement of the fragments takes place.

Cases of fracture through the head of the radius, complicated with other injuries, have been recorded by Malgaigne, Bryant, Stimson, Packard, Hamilton, Adam, Hulke, and others. In several of these cases the coronoid process was broken, and in some dislocation had also occurred. Dr P. H. Watson showed to this Society some years ago a very interesting example of fracture, involving the upper end of the radius, in both arms, the result of direct violence.

III. Complicated Fractures and Displacements.

Case VIII.—An adult had excision of the elbow performed on account of anchylosis and deformity, the result of an injury. In this case there was a fracture through the base of the olecranon, entirely separating it from the shaft of the ulna. This process remained in position in the olecranon fossa, its base, where fractured, being rounded off by absorption. The upper end of the shaft of the ulna, which had also lost its sharp edge by absorption, and the head of the radius, were displaced forwards, and firmly attached by fibrous bands and osseous material to the anterior aspect of the humerus immediately above the condyles.

Case IX.—An adult. Elbow-joint excised on account of deformity and anchylosis following an injury. The parts removed show a transverse fracture through the lower end of the humerus, a little above the condyles, with displacement downwards and forwards of the lower end of the upper fragment. This fragment is firmly united by osseous material to the anterior aspect of the radius and ulna, and to the anterior aspect of the lower fragment and condyles of the humerus. The olecranon is displaced downwards, and has formed a new articular surface for itself immediately below the olecranon fossa. The head of the radius is in its normal position as regards the ulna, but it articulates with the anterior edge of the external condyle.

Case X.—A boy, about 6 years old, injured his elbow by a fall, and this was followed by anchylosis, making the arm very useless. Excision of the joint was performed, and an examination of the parts excised disclosed an oblique fracture through the condyles of the humerus, completely separating the outer half of the epiphysis, together with the external condyle. This portion of bone was displaced upwards and backwards, and also slightly rotated outwards, and was attached by osseous material to the posterior aspect of the humerus immediately above the articular surface. The olecranon fossa was filled up by osseous material, and the olecranon itself was displaced slightly backwards from its normal position by this new material. The posterior margin

of the head of the radius was displaced a little upwards and forwards, its posterior margin resting against the epiphyseal line from which the external condyle had been separated.

CASE XI.—An adult. Preparation obtained from the dissecting-room. The olecranon is displaced upwards and backwards. The head of the radius is also displaced upwards and backwards, and has pushed before it a portion of the outer condyle, which has been broken off. This separated condyle is firmly adherent to the posterior aspect of the humerus, immediately above its normal position. The coronoid process has apparently also been broken off near its base, and is drawn upwards and attached by osseous material to the anterior aspect of the humerus.

CASE XII.—From the University Anatomical Museum. The elbow-joint of an adult anchylosed by osseous material in the semi-flexed position, shows an oblique fracture through the condyles of the humerus separating the external one. This separated condyle is displaced downwards and forwards, and is anchylosed to the ulna. There is also a fracture through the upper end of the radius which is firmly united, and this bone is also united by osseous material to the anterior aspect of the ulna. The radius and ulna have been fractured through their middle thirds, but are firmly united. The forearm is fixed in the semi-prone position.

Cases VIII., IX., and X., although complicated as regards the injury to the bones, were not attended with external wounds of the soft parts, and they well illustrate that the bones forming the elbow-joint may be broken or displaced in various ways without the accident being "compound" in its nature. In regard to Cases XI. and XII. there is no history as to whether or not the injuries were "simple" in their character.

The cases and preparations which have been described in this paper cannot be used with much success in assisting our diagnosis of recent injuries, because they are all examples of old-standing accidents, and the parts have undergone alterations, owing to displacements, adhesions, growths of new bone or atrophy of bone, or of several of these combined; but I would suggest three points for consideration in connexion with them.

(1.) That injuries to the bones of the elbow-joint may be complicated, making their exact diagnosis difficult when there is no external wound communicating with them, and their treatment, even with care, not always satisfactory as regards the after mobility and usefulness of the joint.

It is well known how generally injuries to the elbow-joint are followed by considerable swelling, which adds to the difficulty of correct diagnosis, unless the case is seen before the swelling takes place, and it is therefore a wise precaution to give a guarded prognosis as to the probable future condition of the joint. Some of the cases described clearly show that even with careful and recog-

nised treatment a perfect result can scarcely be expected after certain complicated injuries.

(2.) That the secondary changes which take place after some injuries to the elbow-joint further complicate the case, add to the unsatisfactory result, and make ordinary treatment insufficient.

Perhaps the most important change—and it is well illustrated in many of the specimens—is the development of osseous structure which not only unites the bones or separated portions of bones together, but also mechanically interferes with the movements of the joint. The rapidity with which this change takes place after injury is more rapid than I think is usually supposed, and more particularly in the case of young people, and I have seen cases in which, within a very few weeks after the injury, osseous material was largely developed.

Atrophy of bone, and absorption by the pressure of one displaced bone working upon another, are also well illustrated in some of these preparations; and it is not difficult to understand why atrophy and complete non-union should occur when the head of the radius is entirely separated, as in the case related. Adhesions of osseous material or of firm fibrous structure must always act in increasing the stiffness of the joint, and such changes can rarely be removed without operative interference.

(3.) It having been proved that the complicated nature of some cases of injury to the bones of the elbow, together with the secondary changes which follow them, results in a stiff or deformed joint and more or less useless arm, it is natural to ask, Can any other than the ordinary and recognised treatment be employed which will give a better result? I answer this question by suggesting that, with the aid of careful antiseptic treatment primary operative interference should be more frequently employed than it has hitherto been in certain of these cases. This second question arises out of the first. In what cases should you interfere by primary operation? My reply is, that in any doubtful case as regards exact diagnosis, I would advise that the patient be put under the influence of chloroform or other anæsthetic, that an attempt be made to replace any bones fractured or dislocated, and if this is successful, it should then be tested if these bones remain reduced when the arm is placed in proper position, and simple pads and a bandage or a splint applied. If the bones can be so retained, I would not suggest any operative interference, but would retain the appliance for about two weeks, re-adjusting it from time to time, if necessary. At the end of two weeks careful movements should be employed once every two or three days, with the view of preventing adhesions, but some appliance should be continued, except when these movements are being practised, for other three weeks. It is specially important to practise these movements early in young patients. Should the surgeon not succeed in replacing the bones, or should they not be retained by position and appliance after

being replaced, I would then advocate opening into the joint with antiseptic precautions, and either removing or wiring together, according to the nature of the case, displaced or separated bones. I believe that if this were done, a more rapid recovery and a better result would ensue, than if the case was left and time allowed for the secondary changes to take place, and many who practise operative surgery have experienced excellent results after partial primary excisions of the elbow, provided no serious inflammation and suppuration of the joint take place.

In regard to secondary operations for these injuries, an excision of the joint will, in the majority of cases, be required when the condition of the articulation seriously interferes with the usefulness of the arm, and further mobility or a better position of the limb is desired by the patient. There are, however, cases in which a partial operation may be sufficient, as, for instance, in displacements or fractures connected with the head of the radius, and there are already on record a few of such operations. In fractures involving the olecranon, which are followed by non-union, adhesions, or displacements, a secondary operation may give a good result, as in the following case, which is not the first of its kind, but it is interesting from the fact that two wire sutures were employed and allowed permanently to remain, as in the operation for fractured patella, practised by Sir Joseph Lister.

Case XIII.—A boy, æt. 16, was admitted into my wards upon the 30th of June 1884. About five months before admission he received a kick upon his elbow from a horse. He was seen by his medical attendant, who recognised a fracture of the olecranon, and applied a splint so as to keep the arm and elbow in the extended position. This splint was kept on for several months. When admitted, the elbow was fixed in the straight position, there was an interval of fully half an inch between the two fragments, the upper one being drawn up and fixed by osseous material to the posterior aspect of the humerus immediately above the olecranon fossa. There was only slight movement in the elbow-joint, its surfaces being united by firm fibrous adhesions. Having on the 3rd of July placed the patient under chloroform, I forcibly broke down the fibrous adhesions and flexed the elbow. I then made a free incision over the olecranon and upper end of the ulna, so as to expose the fractured ends, and having first, by means of a chisel, separated the osseous attachment of the upper fragment to the humerus, I sawed a thin slice off each portion so as to raw their surfaces. When the elbow was placed in the semi-flexed position, the two halves of the olecranon were separated fully one inch, and I therefore found it impossible to bring into contact their rawed surfaces. Being anxious to retain the elbow in this flexed position, I cut across the greater part of the triceps muscle

about two inches above its insertion, and when this was done I found I could get the fragments together. Two silver wire sutures were next passed through both fragments, and the ends of the wires having been twisted so as thoroughly to secure the portions of bone in their place, were flattened down, and therefore they did not cause any projection. The progress of the patient was in every way satisfactory, and he was discharged with the wound soundly healed over the wires, upon the 23rd of August. He was able to flex, extend, pronate, and supinate the limb, and its strength was steadily improving when he left the Hospital.

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Dr Hare asked, with regard to the treatment of cases of dislocation of the elbow backwards, whether it would not be well to commence passive movement before twelve days had elapsed. It struck him on seeing the specimens that one was running a considerable risk in leaving the elbow at rest for so long a period. Adhesions might occur which would render the arm less useful. There was one other point in regard to the interesting case of fracture of the neck of the radius. The treatment in a case of that sort must always be a matter of some difficulty. The forearm, he understood, was fixed midway between supination and pronation. Would this bring the fragments of bone into their proper relation? The shaft of the radius would be apt to pass somewhat into the position of pronation. Would it not be preferable to put the arm in a position of acute supination?

Dr Duncan said that Mr Annandale's paper, though extremely valuable in so far as it established pathological facts, did not lend itself to discussion or remark. The very rare injuries with which he had principally dealt—as fractures of the coronoid process and head of the radius—were accidents very difficult to recognise during life, and came under the notice of the surgeon chiefly in museums. He thought it not at all improbable that they had fractures of the coronoid oftener than was supposed. It would be observed in one of the specimens that merely the top of the coronoid was broken off in passing over the end of the humerus, not the articular surface. This, he thought, might not be uncommon in such cases. He should be inclined, in answer to what had been said, to maintain the most absolute immobility for at least a fortnight or a few days longer. The more complete the immobility at first, the less risk of inflammation and later adhesions, and the more likely was it for any such adhesions to occur in their proper positions. Moreover, at the end of three weeks there was ample time to get rid of improper adhesions. In very young children ten days of absolute immobility might be sufficient, but in adults the joint might be kept immobile for three weeks. The movement afterwards should be of a gentle though effective character. The method

he adopted was to keep the limb in a certain position, usually at right angles, for nearly three weeks, then to extend it gently and with the least possible pain, and, if necessary, to put on extension for the night, to flex it again in the morning, and keep it so till night, when it was extended, and so on. The usual method was to pump at the arm, which irritated and tended to keep up inflammatory mischief. The least painful movement was the most effective.

Professor Annandale, in replying, said he agreed with Dr Duncan on the point that had been raised by Dr Hare. Passive movement of the arm ought not to be begun before a fortnight had elapsed. He thought also that Dr Hare had not quite understood him in regard to the treatment of the case of fracture of the neck of the radius. The patient was sent into hospital with the arm midway between supination and pronation, but it was afterwards put in the supine position with a splint on the anterior aspect.

2. *Dr F. M. Caird* read a NOTE ON A TUMOUR OF THE TESTICLE, which will be found in the Appendix at the end of this volume of the Transactions.

Meeting IV.—February 4, 1885.

Dr H. D. LITTLEJOHN, *President, in the Chair.*

I. ELECTION OF ORDINARY MEMBERS.

The following gentlemen were elected Ordinary Members of the Society:—Edward M'Callum, M.A., F.R.C.S. Ed., L.R.C.P. Ed., Edinburgh; T. Edgar Underhill, M.B., C.M., F.R.C.S. Ed., Tipton Green, Staffordshire; John Struthers Stewart, L.R.C.P. & S. Ed., Edinburgh.

II. EXHIBITION OF PATHOLOGICAL SPECIMENS.

1. *Dr F. M. Caird* showed a series of specimens of DISEASES OF THE TESTICLE. The specimens were prepared in the surgical and anatomical departments of the University, after the method devised by Professor Hamilton of Aberdeen. The testicles were injected with carmine or Prussian blue, hardened in spirit, washed, soaked in gum and sugar, frozen and cut in a microtome, and the sections then mounted in glycerine jelly between plates of glass. In this way entire transverse and longitudinal sections, illustrative of sarcoma, chondro-sarcoma, tubercular, syphilitic, and cystic

disease of the testicle were obtained. The chief difficulty encountered in cutting the organs lay in the variable density of the new formations, so that it was not easy to get uniformly thin sections suitable for microscopic examination. Much valuable aid in mounting the preparations had been afforded by Mr James Simpson of the Anatomical Museum.

2. *Dr MacGillivray* showed (*a.*) a MELANOTIC SARCOMA occurring primarily in the lymphatic glands. (*b.*) A RECURRENT TUMOUR from the same case. The patient, a young married woman, had an ingrowing toe-nail. As a result the glands in Scarpa's triangle enlarged. As they continued to do so after her confinement it was thought advisable to remove them. This was done with some difficulty, as the saphena vein was involved and had to be cut. They were at first thought to be simply enlarged and irritated glands, but on section and microscopic examination it was seen to be a well-marked case of melanotic sarcoma. The patient remained well for about four months. At the end of that time she complained of intense pain in the left iliac region, above Poupart's ligament. Peritonitis with diffuse swelling was present. After about a fortnight she recovered, but a hard mass about the size of an orange remained, with a tendency to further increase in size. After another attack of pain and peritonitis it was agreed that there must be a recurrence of the disease, and that it would be better to remove it. On cutting down above Poupart the tumour was found over the external iliac vessels, and adherent to them. The peritoneum practically formed a sort of capsule round a portion of the tumour. It was therefore opened during the removal of the tumour. The patient never absolutely recovered from the shock. Subacute peritonitis set in, and she died of exhaustion. The wound remained aseptic throughout, and was almost healed at the time of death. The points of interest in the case were the primary occurrence of sarcoma, more particularly melanotic sarcoma, in the lymphatic glands, and the peritonitis set up by an extra-peritoneal tumour. The foot was carefully examined, but showed no sign of malignant mischief.

3. *Dr Allan Jamieson* showed two drawings. (*a.*) The first represented a curious and uncommon affection of the skin, LICHEN MARGINATUS, on the back of a man, æt. 33. The eruption first appeared at the beginning of winter, and disappeared under local treatment. It recurred in the end of January. The disease began as little rounded spots, the centres fading away and becoming yellowish. It had been described under various names,—by the French as pityriasis maculata et circinata, and by Kaposi as a form of ringworm in which the disease spread with rapidity. Kaposi, however, made no mention of any fungus, and a careful

examination of the scales from the case showed no fungoid growth. The disease seemed chiefly to affect adults, and more especially the back and chest. It was accompanied with considerable itching, and did not appear to be associated with any disorder of health. Local treatment alone seemed to affect it. Lassar's paste acted successfully before, and the same treatment was being applied now. (*b.*) The second drawing represented a very rare result, INTENSE BLACK PIGMENTATION occurring after PEDICULOSIS of the skin. It showed the front of chest and left shoulder of a man affected with pediculi for an indefinite period. He was a man of darkish complexion, and the pigmentation which followed the scratching of the skin was very marked.

III. ORIGINAL COMMUNICATIONS.

1. ON A CASE OF TAPPING OF THE PERICARDIUM, AND ON TAPPING OF THE PLEURA IN THE TREATMENT OF CARDIAC DISEASE.

By T. GRAINGER STEWART, M.D., F.R.C.P. Ed., F.R.S.E., Physician in Ordinary to the Queen in Scotland, Professor of Practice of Physic in the University of Edinburgh.

IN this communication I purpose to bring under the notice of the Society two subjects, which, although neither novel nor recondite, seem to me of considerable practical importance.

I. A CASE OF PERICARDITIS.

On Monday, 12th May 1884, I found in Ward XXIII. of the Royal Infirmary a young man who was evidently dangerously ill. He was very pale, his lips somewhat livid, his face covered with perspiration, his respiration was rapid and shallow, his pulse extremely quick and feeble. On examination of the heart, slight bulging was observed, while the area of dulness was markedly increased in all directions. On auscultation the sounds were indistinct, but no friction could be made out. On examining the lungs, I found marked dulness at the left base posteriorly, with diminution of vocal fremitus and of respiratory sound, and my resident physician, Dr Charles Brown, informed me that he had introduced an exploring needle, but failed to get any fluid from the pleura. I found that the patient had been admitted on 10th May, with a note from my friend, Dr Longmuir of Bathgate. The family history was favourable. The young man was 17 years of age, employed as a pupil teacher; he had been quite well until November 1883. He had then got a chill, followed by inflammation in the chest, which confined him in the house for two months. In

January, however, he had been able to resume his duties, although he did not feel quite well. In the middle of April his illness again became more formidable, and early in May he consulted Dr Longmuir, who recommended his removal to the Infirmary. He was accordingly admitted on the 10th May.

His temperature on admission was 102°·8 F., his respirations 35, his pulse 160, pretty regular. The house physician, finding the dulness at the back of the chest, made the exploratory puncture to which I have already referred, and ordered various medicines, and on Sunday, the 11th, the temperature was 100°·2, the respirations 32, and the pulse 98. But on the following morning he was worse. There was great oppression in the chest, the temperature was 101°, the breathing more rapid, and the pulse so quick, feeble, and irregular, as to be uncountable.

From the facts which were elicited, it was clear that in addition to the thoracic inflammation, which had existed during the winter, pericarditis with considerable effusion had set in. I directed that the patient should be kept perfectly quiet, should have a good supply of nourishing soup and milk, and brandy in frequent small doses. I visited him again at 5 P.M., and found the pulse hyperdicrotic, markedly affected with the respiration, but firmer, more regular, less rapid than it had been in the middle of the day. Tracing A shows the characters of the pulse at that time. During the

A.

evening the condition again got worse, and I was sent for at 10 P.M. The pulse was then threatening to fail. A tracing (B) was

B.

obtained with difficulty, and it showed characters approaching those of the pulsus paradoxus of Küssmaul. The area of dulness, although considerably enlarged, was much less than I have often seen it in cases of pericarditis, but the danger to life was so obvious from threatened failure of heart, that I deemed it right to decide upon operative interference. I introduced the needle of a Wood's syringe towards the left margin of the area of absolute dulness in

the fifth intercostal space, and found that a reddish blood-stained serum flowed freely. A fine needle connected with an aspirator was then introduced, and two ounces of fluid were withdrawn. Distinct relief followed this operation. A tracing of the pulse (C)

C.

taken at 11 P.M., showed a much better character, while the patient expressed himself as being distinctly relieved.

On Tuesday, the 13th, he was markedly improved at the midday visit, but towards evening the heart again began to fail. A tracing obtained at 7 P.M. showed an unfavourable character, and although it improved somewhat under free use of brandy, it seemed advisable again to tap. Four ounces of blood-stained fluid were, therefore, drawn off from a point, close to that at which the previous puncture had been made. Improvement immediately manifested itself, and even more distinctly than on the previous occasion. During the 14th, his condition was more favourable, the temperature 101°·8 F., the respirations 23, the pulse 105, and quite regular. The area of dulness was distinctly diminished, and friction was audible over the whole surface of the heart. Digitalis was given in 10 m. doses of the tincture, three times a day; the alcoholic stimulant became less necessary; the patient continued gradually to improve. On 7th June, friction had disappeared, and on 2nd July the patient was dismissed cured.

I have not thought it necessary to trouble the Society with many details of this case, because for the present purpose such details are unnecessary.

It is impossible to assert positively that the tapping saved the life; possibly the patient might have survived without it; but I think he would have died. It is true that one often deems it unnecessary to resort to operation even when effusion is more extensive than it was in our case, but the state of the pulse so alarmed me as to immediate danger to life, that I thought it necessary to interfere. The operation is certainly very rarely required. Only once before in my practice have I thought it necessary to perform it, and in that case a complication with endocarditis rendered the patient's condition hopeless, and the relief was only temporary.

But, while rarely required, paracentesis pericardii has a well-established place. It is unnecessary for me to go into the history of it in detail, but I may remind you of its introduction by Romero of Barcelona in 1819, of its being performed by Schuh in one of Skoda's cases, of Trousseau's successful performance and warm

advocacy of it, and of the valuable evidence in its favour supplied by Dr Clifford Allbutt of Leeds. My friend, Dr Philip, has gone over the literature of the subject for me, and has compared the statistical tables prepared by different writers. Three authorities have collected series of cases, namely, Dr Hindenlang of Freiburg, Dr Roberts of Philadelphia, and Dr Samuel West of London. I have written their results on the table.

HINDENLANG (*Deut. Archiv f. klin. Med.* 1879).—Total number of cases reported, 50. In some of these tapping was performed more than once, thus making really 65 tappings, of which were

Successful.	Unsuccessful.
21	44

ROBERTS (*Paracentesis of the Pericardium*, Philadelphia, 1880).—Total number of cases reported, 60; of which

Successful.	Unsuccessful.
24	36

WEST (*Transactions of the Med. Chir. Soc.*, London, 1883).—Total number of cases reported, 80; of which

Successful.	Unsuccessful.
31	49

(*N.B.*—In almost every case, even in those ultimately unsuccessful, temporary relief followed the operation.)

Taking West's statistics as basis—

Successful.	Unsuccessful.
31	49

there have to be added from Hindenlang 3 cases, all of which were unsuccessful; and from Roberts 7 cases, of which 3 were successful and 4 were unsuccessful; and from other sources (not included in statistics) 7, of which 4 were successful, and 3 were unsuccessful.

Thus, from all sources we have 97 cases, of which

Successful.	Unsuccessful.
38	59

With results such as these, it is clear that the operation deserves recognition as justifiable practice in certain cases.

What are the indications for its use?

1. It should be tried wherever life is imperilled by the copiousness of the effusion.

2. It should be tried, even if pericarditis be not in itself dangerous, in any case of considerable pericardial effusion in which the pulse threatens to fail. Whether it be due to inflammatory or

degenerative changes in the cardiac muscle, or to general debility from severe or prolonged disease.

It was upon the second rule that I founded my practice in the case under discussion.

What are the best rules for operative procedure?

1. Exploratory puncture should be made by means of a Wood's syringe or other fine perforated needle, the needle being cautiously introduced at a point where there is absolute dulness and least likelihood of injuring the heart.

2. If serous fluid be found, the fine needle of an aspirator should be introduced at the same point and the fluid drawn off.

3. If purulent fluid be found, either aspiration, or what is probably better, free incision should be resorted to and the pus evacuated. The splendid results obtained by the latter plan of treatment by Dr West and by Professor Rosenstein of Leyden must satisfy any one who reads their papers of the value of this method.

4. As to the quantity to be drawn off, opinions are somewhat contradictory. If the fluid be purulent, it is obviously desirable to remove the whole of it as speedily as possible; if it be serous, I think that this rule does not necessarily hold. While admitting that there is plenty of evidence to show that the pericardium may be emptied or almost emptied without danger to the patient, it appears to me that only a sufficient quantity to give relief should be removed. It is a sound principle that in dealing with vital organs only the minimum amount of interference required should be had recourse to, and especially in cases which threaten failure of pulse is this precaution necessary. It is conceivable that the sudden removal of considerable pressure from the surface of the heart might sometimes lead to a fatal syncope, while the removal of a small quantity of fluid would involve no such danger. You are familiar with the occasional occurrence of syncope when paracentesis of the pleura is being performed, and whatever may be the explanation of this fact, it seems quite as likely to occur in connexion with the pericardium. I therefore prefer, as at present advised, to draw off only a small number of ounces, and, if necessary, to repeat the operation rather than to adopt the method recommended by the majority of authorities, and draw off a large quantity at once.

5. At what point should the puncture be made?

It is not very important what point is selected for puncture, so long as the operation is performed with caution. Obviously wounding the heart is to be carefully avoided, notwithstanding the fact that it has been wounded, and even penetrated, without seriously bad effect. I should insist upon the puncture being made where there is absolute dulness, and should prefer the fifth interspace as much to the left of the sternum as possible. By such a rule we most avoid risk of injuring the heart.

One other point in connexion with this case deserves attention,

namely, the character of the fluid which was drawn off. It was markedly bloody, much more so than my experience of post-mortems in cases of the kind had led me to expect. One would not be surprised at finding the serum bloody in a case of pericarditis associated with purpura or scorbutus, or even in one associated with malignant disease, but I was not prepared for the appearance of such fluid in this case. Küssmaul, however, has recognised the fact that bloody serum is common in pericarditis, and mentions one case in which, desiring to tap the pericardium, he got a straw-coloured serum, and, suspecting that he had only reached the pleura, pushed the needle further in and got the characteristic pericarditic fluid. I would, therefore, warn anyone who is performing the operation that he need not be startled if he finds the fluid of a reddish colour.

II. Tapping the Pleura in the Treatment of Cardiac Disease.

The subject of tapping the pleura in cases of cardiac disease is one which I should scarcely have thought of bringing separately under the notice of the Society, seeing that it is so obviously suggested by the condition of such patients, and might, therefore, be assumed to be very generally adopted. But I find that it is little used by practitioners, even such as have become familiar with tapping for pleurisy, and in four recent standard works upon diseases of the heart, into which I have looked for reference to this special point, I find no mention of the procedure. In these circumstances I hope that the Society will not consider that I am wasting its time in drawing attention to the subject.

There are two classes of cases which should be recognised—first, the obvious cases of hydrothorax in which, along with cardiac disease, there is great dyspnœa constantly present, although occasionally aggravated by severer paroxysms. With these symptoms there is also extensive dulness on percussion in the lower part of the thorax, absence of fremitus and of respiratory sounds, and not unfrequently œdema of scrotum or general anasarca.

But there are other cases in which the physical signs are less distinct. There is again cardiac disease with persistent dyspnœa, the dyspnœa often extremely distressing. There may or may not be some degree of general dropsy. On examination of the chest, there is little dulness, not more than an inch or two inches at the base of the pleura on both sides, or only on one, the vocal fremitus is not lost though diminished, and the respiration, although indistinct, is audible. At the upper margin of the dulness crepitations are often heard, and the symptoms and signs might be assumed to indicate a mere œdema, or a quite unimportant effusion into the pleura. Nevertheless, when paracentesis is performed, fluid flows

readily, and perhaps twenty, thirty, or even more ounces are removed, with the effect of immediately relieving the dyspnœa, improving the heart's action, and, consequently, dispelling any general dropsy which may have been present. Now, when I first adopted this practice, a good many years ago, I used to wonder how so large a quantity could be present in the thorax with so little dulness on percussion. The explanation appears to me to be that, in these cases, a depression of the arch of the diaphragm takes place either from changes in the muscular fibre of that structure itself, or from long continued pressure downwards by effused fluid. If such depression exists, it is obvious that the capacity of the thorax may be greatly increased.

It is unnecessary to say anything about the mode of performing the operation. The only point on which I should insist is the preliminary exploration with the injection syringe. I always carry such an instrument in my pocket, and in any doubtful case can satisfy myself at once as to whether fluid is present or not. If it be present, I introduce the needle of an aspirator, and draw off whatever quantity of fluid may be present. The results obtained are usually very satisfactory, the patient's suffering being greatly diminished, and life being prolonged for months, or even for years.

I may refer to one or two illustrative cases. I saw, with my friend Dr Ronaldson, a gentleman, between 60 and 70 years of age, who had for some years had a tendency to bronchitis with distinct mitral incompetence and dilatation of the heart, a good deal of œdema, and other signs of backward pressure in the circulation. An attack of bronchitis had increased the embarrassment of the heart, and the condition became very alarming. There was great dyspnœa, slight dulness at the right base, marked dulness at the left. After trying digitalis, carbonate of ammonia, and other medicines, without getting rid of the dyspnœa, the left pleura was tapped, and thirty-eight ounces of fluid withdrawn. Steady improvement set in, and in a short time the patient was able to walk on the level without dyspnœa. Both Dr Ronaldson and I were satisfied that this good result could not have been otherwise obtained.

The next case which I shall mention is that of a man who was for upwards of a year under my observation as a hospital patient. He was nearly 70 years of age, free from important disease, excepting pretty widespread atheromatosis, with mitral incompetence and cardiac dilatation. He suffered from persistent dyspnœa, with occasional exacerbation of great severity. Examination of the lungs showed only a limited area of dulness, from an inch to an inch and a half at the bases, more marked on one side than the other. I drew off the fluid from one pleural cavity, with the effect of immediately, and for a considerable time, relieving the respiration. The fluid gradually again accumulated, and it became

necessary to tap the pleura once in a fortnight or three weeks. By this means the patient was kept alive and in tolerable comfort for more than a year, and ultimately died from another cause, to which I need not refer.

Another illustration is afforded by a case in which, in addition to cardiac disease, there was general dropsy, such distension of the scrotum and prepuce as made walking impossible and micturition extremely difficult. Diuretics and other appropriate remedies failed to give any relief, and puncturing the distended skin seemed to be hazardous. Finding effusion in the pleura, I proceeded to draw it off, and from that time all the symptoms became relieved, and the dropsy gradually disappeared, so that the man was able to return to his work as a labourer.

I had recently in the wards a patient suffering from mitral disease, and in a state of extreme dyspnœa. On examination I found œdema of scrotum, some swelling of legs, and a little dulness at the base of the thorax posteriorly. Having made sure of the presence of fluid in the usual way, I directed the house physician to draw it off. He removed twenty-eight ounces. The breathing was immediately relieved, and the dropsy speedily began to subside.

These instances may suffice as illustrations. I might have given them in greater detail, but consider that a brief statement is sufficient.

It is often worth while to remove the fluid from the pleura even in cases in which no prolonged benefit is to be looked for. In illustration, I may refer to a case which I saw along with my friend, Dr Malcolm, a case of mediastinal sarcoma, which was attended by effusion into the pleura and great dyspnœa. The pleura was tapped on several occasions, and great relief was experienced on each occasion, life being prolonged for a few weeks in consequence. It is unnecessary to cite other cases in illustration of this point.

Such operative interference does not always succeed. The heart may be so much affected, or the lungs so œdematous, or other complications may exist of so formidable a nature, that it is impossible for the removal of fluid to prove serviceable; but I have never seen a bad effect from it, unless, perhaps, a little temporary disturbance of respiration.

The President expressed the feeling of indebtedness of the Society to Professor Grainger Stewart for his interesting and valuable paper. He intimated that he had received notes of apology for absence from Drs Balfour, Brakenridge, and Wyllie, as also from Professors Gairdner and M'Call Anderson of Glasgow. The latter gentleman had in addition sent a short note of his views on the operation of paracentesis pericardii, which he would now call upon the Secretary, Dr James, to read.

Dr James, Secretary, read the following remarks, which had been sent by Professor M'Call Anderson:—I am fully sensible of the honour which the Council of the Medico-Chirurgical Society has done me in inviting me to be present, and to take part in the discussion upon paracentesis of the pericardium. I regret, however, that my professional engagements prevent me from accepting the invitation, and all the more as I shall thus miss the opportunity of listening to the paper of my friend Professor Grainger Stewart. I am not aware of its exact scope, nor of the points to which attention is specially to be directed; but I presume that, among them, the dangers connected with, the indications for, and the mode of performing the operation of paracentesis pericardii will be included. I may therefore be allowed to say that I think the dangers of the operation have been exceedingly over-estimated, and that if carried out with reasonable care by a skilful physician, it is probably not much more hazardous than the same operation upon the pleura. Cases necessitating such interference are in my experience by no means common, but I would not hesitate if I found that an accumulation of fluid in the pericardial sac was seriously embarrassing the circulation, and that there appeared a risk of its contributing to a fatal issue to the complaint were such active treatment to be dispensed with. I presume that no one nowadays would think of operating with an ordinary trochar and canula, but Southey's trochar is admirably adapted for the purpose. If, however, it does not succeed, the aspirator should be employed, Potain's being the one which I am in the habit of using. The needle should be small, not exceeding, in the first instance at all events, from $\frac{1}{8}$th to $\frac{1}{10}$th of an inch in diameter; and, as I have myself frequently suffered from leaving the testing of the instrument to others, I may add that we should always *personally* satisfy ourselves that it is in perfect order. Should there be any localized bulging I would probably select that point for the puncture, but if not, the fourth or fifth intercostal space, about 2 inches to the left of the sternum, would usually be chosen. The patient should be in the recumbent position unless the embarrassment of the respiration prevents it. The needle should be first embedded in the subcutaneous cellular tissue, and then the stop-cock being opened so as to complete the vacuum, it is pushed inwards and upwards until the fluid in the pericardium is reached. It is of importance to draw off the fluid very slowly, so as gradually to remove the pressure from the heart, and thus to avoid the risk of syncope. The following case illustrates some of the points to which I have thus briefly alluded:—On the 22nd of July 1879, a lad, 17 years of age, was admitted into the Western Infirmary complaining of severe cough and general dropsy. The cough had troubled him for six years, being always worst in winter, and during the last four winters he had several very severe attacks of hæmoptysis. During the previous winter the cough increased so much that he had to give up

work about Christmas time. Two months later shortness of breath became a prominent feature, and his legs and body became swollen, the dropsy never afterwards disappearing. When admitted he breathed with difficulty, respiration being accompanied by audible wheezing. He had frequent and violent fits of coughing, and his face had a livid hue. On examining the chest anteriorly, the pericardial region was observed to be decidedly fuller than the corresponding part of the right side, and there was some bulging of the intercostal spaces; and, along with this, there was great dulness and resistance upon percussion. The dulness extended 2 inches to the right of the middle line, and round into the left lateral region; it reached upwards nearly to the clavicle, beneath which, however, there was a limited area of resonance, and the lateral limits of the dulness were much less at the upper than at the lower part of the chest. Posteriorly, percussion was clear, except at the left base, where the physical signs pointed to moderate pleuritic effusion. The exact position of the heart could not be made out, nor could the apex beat be felt. The pulse was rapid (112), very small, thready, and irregular. Harsh sonorous râles were heard all over the chest, but there was no complaint of pain anywhere. The feet and legs were œdematous, and there was slight distension of the abdomen, but no evidence of fluid in it. The liver was somewhat enlarged and tender, and there was some pain on pressure over the left hypochondrium. The urine was scanty, and contained a trace of albumen and of bile. At a consultation held on the 30th July, the diagnosis of pericardial effusion was confirmed, and it was resolved to remove the fluid by means of the aspirator. The spot selected for puncture was the fifth intercostal space, about an inch to the right of the nipple line. The skin having been frozen with æther spray, a medium-sized trochar and canula connected with Potain's aspirator was used, and 38 ounces of a light, straw-coloured fluid withdrawn. When the instrument was first introduced, it was evidently not in contact with the heart, but during the latter part of the operation the cardiac impulses distinctly affected the canula. There was no complaint of pain or of faintness during the operation, and after it the breathing became decidedly less laboured; and very soon there was a marked improvement in his appearance, which, previous to the operation, was characteristically cyanotic. That night he slept better than he had done for a long time previously. On the second day after the operation (1st August), it was noted that the area of dulness was considerably diminished, the diminution being most marked at the upper part and to the right. The heart sounds were now very distinct and more superficial, and the pulse was much stronger and more regular, though still frequent. The râles had in great measure disappeared from the chest, and the symptoms of passive congestion had to a large extent subsided. Towards the latter part of September he began

to complain of pain in the abdomen, which now contained fluid; the legs were again œdematous, and the cough had become much more troublesome, but there was no distinct increase in the cardiac dulness. On the 30th of October, as the fluid in the abdomen had become excessive, it was removed with a trochar and canula. Its colour was dark amber, and it coagulated in a basin while being drawn. It measured 290 ounces. This, however, gave only temporary relief, as it soon collected again, and the dropsy of the subcutaneous cellular tissue became excessive. On the 24th November severe diarrhœa set in, and he died on the morning of the 30th. Unfortunately a post-mortem examination was not permitted. Although this case ultimately terminated fatally, there can be no doubt that the tapping of the pericardium gave decided relief, and prolonged life.

Dr Byrom Bramwell expressed the great pleasure with which he had listened to Professor Stewart's most important and practical paper. Amongst the many points of interest, the following appeared to him to be some of the chief. The case of pericarditis was interesting inasmuch as it did not, so far as he had gathered, appear to be due to rheumatism or any of the ordinary causes of pericarditis, but to have followed a chill. (Possibly it was secondary to some other inflammatory conditions within the thorax, which had resulted from the chill.) The hæmorrhagic nature of the effusion was also of interest. Professor Stewart had given some authorities to show that the effusion was more bloody in the pericardium than in the pleura. He was not prepared to deny that this was the case, but hæmorrhagic pericarditis was in his experience rare in the post-mortem room, except in cases of tubercle, cancer, or some general pathological condition, such as purpura. In his experience the effusion in cases of pericarditis was much the same as in cases of pleurisy. The main interest of the case was, however, the bearing it had on paracentesis pericardii. Professor Stewart was to be congratulated on the happy and successful issue of his treatment. Many of the points in the case went, he thought, far to establish the rules he had laid down for the performance of the operation. There were two points he would like to allude to. The point of puncture which Professor Stewart had advocated was not usually recommended. Almost all authorities agreed that the puncture should be made in the fifth interspace, midway between the nipple and the left border of the sternum, or if the heart were enlarged or tied down by adhesions, in the sixth or seventh interspaces, as recommended by Sibson. Perhaps Professor Stewart would be so kind as to inform the Society on what grounds he preferred the situation which he had recommended. Again, with regard to the amount of fluid which should be withdrawn, Professor Stewart advocated the withdrawal of a small amount, and directed attention to the fact that he (Dr Bramwell) had advised the removal of the whole. But he had especially guarded himself

against the risk which Professor Stewart feared, by stating that if any indication of syncope or any other untoward symptom should arise, the aspiration should be at once stopped and the needle withdrawn. He took it the conditions were not altogether similar to those met with in pleuritic effusions. Some patients during the aspiration of a pleurisy complained of pain, dyspnœa, and faintness, and death had sometimes occurred. Dr Bramwell thought that in most of these cases the lung was probably tied down by adhesions, and that the disturbance caused by the production of a powerful vacuum, with probably in some cases effusion of blood or rupture of adhesions, was the cause of the untoward symptoms. He saw no reason to change his opinion, and provided that the fluid was slowly withdrawn, and that no unfavourable symptoms arose during the operation, he saw no reason why the whole of the fluid, if possible, should not be withdrawn. At the same time, he was prepared to agree with Professor Stewart that the withdrawal of a small quantity of the fluid might be sufficient. He had more than once known the withdrawal of a small quantity of pleuritic fluid followed by rapid absorption, in consequence, probably, of the over-distension, with resulting pressure upon the veins and lymphatics, on the pleuritic wall, being removed. Most authorities were agreed that when the amount of fluid was so considerable as to seriously embarrass the action of the heart, paracentesis pericardii should be performed. Professor Stewart went much further, and recommended paracentesis should be practised even when a small amount of fluid was present, and when symptoms of failure of the action of the heart were prominent. The desirability of operating under such circumstances appeared to Dr Bramwell to be doubtful. He did not see how a small amount of fluid in cases of simple pericarditis could *per se* be the cause of cardiac failure. Unless the fluid were acting injuriously mechanically he did not see any reason for interference. The difficulty was to be certain of the diagnosis when a small amount of fluid only was present. The fact that Professor Stewart had only operated twice showed that cases demanding operative procedure were rare. The preliminary puncture with a hypodermic syringe had not, so far as he knew, been so prominently advocated before. He thought the point was one of great practical importance. It was not always easy to distinguish between an effusion into the pericardium and a dilated heart. In such cases puncture with a *clean* (aseptic) hypodermic syringe would not do any harm even if the case was one of dilated heart. The preliminary puncture with a fine hypodermic syringe would therefore in some cases be of great diagnostic use. With regard to the second part of the paper, he agreed with Professor Stewart as to the desirability of operating when any considerable quantity of fluid was present. So far as he knew, it was the general practice to puncture the pleura in cases of hydrothorax when anything like 60 ounces of fluid was thought to be present. He did

not mean to say that this quantity of fluid was never found post-mortem, but it must be remembered that in some cases of this description there was reason to suppose that some of the fluid had been poured out during the last day or two of life—sometimes, he thought, during the last few hours of life. He was not previously aware of the fact that so much as 60 ounces of fluid could be present in cases of hydrothorax, with only one inch of dulness over the base of the lung. Such a condition must, he thought, be rare. When such a large amount of fluid as 60 ounces was found post-mortem, there was usually, he thought, some evidence of compression of the lung, and therefore presumptive evidence that the extent of dulness was considerably greater than an inch.

Dr Haddon thought it would be well if general practitioners gave their opinions on these questions a little more freely than they usually did. The first time he had seen the pleura tapped was by Professor Stewart in 1866 in the Children's Hospital. It was then customary to talk of tapping the pleura in the very way they talked of tapping the pericardium now. That case made a deep impression on his mind, and in practice he had since tapped very often, and sometimes perhaps rashly, but he always made his preliminary puncture with the hypodermic needle, and he never saw any harm result. In some instances, more particularly in one case which turned out to be a tumour of the lung compressing a bronchus, he had punctured without getting any fluid, but never saw a bad result from so doing. The patients were none the worse for the punctures. As to the operation of tapping the pericardium, he felt he could only theorize, as he had never performed it, but every one ought to have a theory to guide him when he came to have a case requiring this treatment. If he had a case with effusion into the pericardium and none into the pleura, he would do paracentesis of the pericardium, but if the pleura was also the seat of effusion he should empty it first. The point of puncture, he thought, should be left to the operator. He should also withdraw all he could. He had never seen fainting or other untoward symptom arise during the tapping of the pleura, but that possibly was because he tapped early. As to the second part of the paper, he should have thought it would have been a routine practice to remove any fluid wherever it was if it complicated the case. He was glad to hear that preliminary puncture with the hypodermic syringe was a routine practice in the clinical wards, and he did not agree with Dr Bramwell that the fluid in hydrothorax, or at least a large amount of it, was poured out in the last few hours of life. He was quite willing to accept the censure deserved if a case of his went to the post-mortem room with the large quantity of fluid found in cases of hydrothorax.

Dr Affleck was sorry he had not heard the whole of Professor Stewart's paper, but he joined with previous speakers in con-

gratulating him on the happy result he had obtained. The operation of paracentesis pericardii was one rarely resorted to, because pericardial were not so common as other effusions. The results, too, could never be expected to be so successful, because they did not often contemplate tapping until the patient was in danger, and other conditions existed which went far to render the result doubtful. Sometimes pulmonary complications were present, but very often the condition of the heart itself was at fault. The cardiac muscle must often suffer in pericarditis. In the feeble flickering pulse there was evidence of myocarditis accompanying pericarditis, so that removal of the effusion did not always relieve. In Professor Stewart's case the pulse tracings would seem to show that the trouble was mostly caused by the pressure of the fluid on the heart; but in other cases the removal of the fluid had not the same good effect, showing that myocarditis was present. In many cases the operation was successful; in a large percentage it was not. In Hayden's book 13 cases were given in which the operation was successful in itself; in 7 the patient survived. In some instances mistakes had been made. Even Trousseau once mistook an enlarged heart for pericardial effusion. As to the operation, though a great many objections had been raised, he thought that with their present instruments there should be no difficulty in its performance. He agreed with Dr Bramwell that the place to puncture at was not very far from the sternum, where the pericardium might be entered with the greatest safety. He considered that the shock from the puncture should not be forgotten as an element of danger. With regard to the second part of the paper, he thought all of them went on the rule laid down by Dr Bowditch, that when there was mischief arising from fluid in the pleura it was well to operate, although here, as well as in pericardial effusion, rules could not be rigidly adhered to, but cases must be judged on their own merits, and according to the urgency of symptoms.

Dr James Ritchie thought those cases, in which the effusion into the pleura depressed the diaphragm, showed the advantage of having a definite rule as a guide to the amount of fluid that should be drawn off. The pulse should be watched, and if flagging occurred, or a short cough or troubled breathing, the operation ought to be stopped. Were the upper level of the fluid taken as the sole index of the amount of fluid present, probably the operation would be stopped before nearly all the fluid was removed, and in dropsical effusions it was of advantage to remove as much of it as possible. He remembered a case under Professor Stewart's care, in which, although the upper limit of the fluid was low, from 60 to 70 ounces were aspirated.

Dr William Russell hoped that Professor Stewart's paper would help to dispel much of the timidity with which paracentesis of

the pericardium was regarded. He thought paracentesis of the pericardium was not more dangerous in itself than the puncture of any other serous cavity, so long as there was no mistake in the diagnosis, but this difficulty in diagnosis was the reason why practitioners usually approached the operation with some degree of fear. It had fallen to his lot to tap the pericardium in two cases, in neither of which had any unpleasant symptom occurred during or after the operation which might be ascribed to it. Death resulted in both from different causes, but in neither was the fatal issue hastened by the operation. In one case the wall of the ventricle was pricked, but apparently without any harm. Aspiration in cases of hydrothorax (from cardiac disease) did not receive in the special text-books on heart disease the prominence it deserved. He had had many opportunities of judging of its value, and he gave the proceeding his unqualified adhesion, and hoped that Professor Stewart's communication would lead to its adoption by many who did not at present practise it.

Professor Grainger Stewart thanked the Society for its very kind reception of his paper. He had also to thank Dr Claud Wilson for the care he had taken in preparing the notes of the case of pericarditis, and for the pulse tracings which he had taken. He disclaimed anything novel with regard to aspiration in hydrothorax, but as he found the practice was not so common as it should be, he had ventured to bring it before the Society. He valued very much Dr Affleck's and Dr Bramwell's remarks. He thought he was not sufficiently clear on the second rule. He wanted to say that if there was a copious effusion he should tap, but if there was a considerable effusion, not in itself sufficient to cause danger, but if attended with a fading pulse, which he supposed was due to myocarditis, then he should think it his duty to try paracentesis. Given such an indication for tapping (which he had in this case, for he thought there was myocarditis present in it), he should be particularly careful not to draw off too considerable a quantity, because the muscular fibre of the heart might be too weak to admit of this. As to the point of puncture, he agreed with Dr Haddon, but preferred to go outside the nipple line, further away from the heart muscle so as not to injure it.

2. AN OPERATION FOR DISPLACED SEMILUNAR CARTILAGE.

By THOMAS ANNANDALE, F.R.C.S. Ed., F.R.S.E., Regius Professor of Clinical Surgery, University of Edinburgh.

THE pathology of the condition called by that wise old surgeon Hey, of Leeds, "internal derangement of the knee-joint;" by Sir Astley Cooper, "partial luxation of the thigh-bone from the semilunar cartilages," and which is now by some authors termed

dislocation or displacement of the semilunar cartilages, has not yet been thoroughly worked out, as few opportunities occur for the dissection of a joint so affected. It is, however, a clinical fact that one of the semilunar cartilages, usually the internal one, does occasionally become loosened from its attachments; and, in consequence, this body is liable to be displaced either forwards or backwards, and so to interfere with the proper movements of the knee-joint.

Two classes of this displacement are met with: one in which the condition takes place suddenly, as a result of a twist or wrench of the knee; and the other in which the displacement is not so sudden, but appears to depend upon a gradual stretching of the attachments of the cartilage, owing to some effusion into the joints, or owing to some continued strain upon the joint, as is illustrated in connexion with certain occupations.

When the displacement has once occurred in either case, it is liable to occur again; but from my experience, I judge that in cases, the result of a sudden rupture of the ligamentous attachments, which are promptly and carefully treated, the displacement is less likely to recur than in the more chronic ones.

If the condition be not permanently relieved, the displacement of the cartilage takes place more or less frequently in different cases; and sometimes in connexion with the slightest movements of the joint.

The symptoms of this accident, as is well known, also vary in degree in different cases. The movements of the joints may be merely stiffened in one direction, or the joint itself may be firmly locked, and remain so until manipulation returns the displaced cartilage. Two patients have come to me from considerable distances suffering from this condition, and in both the knee-joint had been firmly locked in a flexed position for many hours. Manipulation easily replaced the cartilage, and the movements of the joint were at once re-established.

In all cases of this affection, some effusion into the joint follows the displacement.

The ordinary treatment of a displaced semilunar cartilage is to reduce it by flexion, extension, and manipulation; to apply a splint or elastic bandage, in order to keep the joint at rest, and prevent the displacement from recurring; and, if effusion be present, to employ the usual remedies to promote its absorption. When the accident is recent, I would strongly urge the importance of keeping the affected joint absolutely at rest for two or three weeks, so as to promote the union of the ruptured attachments.

This affection may become so troublesome, owing to the constant recurrence of the displacement, that a patient's occupation and comfort are seriously interfered with; and I relate the following example of such a condition in order to illustrate a new method of procedure which I successfully adopted in connexion with it.

The excellent result obtained in this case encourages me to express the opinion that this, or some similar proceeding, may now become an established means of treatment, when the more simple methods fail to give relief, and to obtain for the patient an useful limb.

CASE.—Thomas M., aged 30, miner, was sent to me from the north of England, on November 1st, 1883, with the following history. About ten months before his admission, he was working in a kneeling position, when he felt something give way in his right knee. He suffered sharp pain, but continued at his work for a few hours. Great swelling of the joint followed, and the pain became much aggravated, so that he could not return to his work, and he had not since worked at his occupation. The condition was treated by rest, blistering, the application of iodine, and various liniments, with the result of reducing the swelling; but the pain still continued, and the movements of the joint were interfered with by something "slipping" in the knee.

On admission, the joint was slightly swollen, and there was a small amount of effusion into its cavity. The patient complained of acute pain in certain movements of the joint, which frequently became locked in the flexed position. He was able, by a little manipulation, to unlock the joint, but the frequency of this symptom made him quite unfit to follow his employment as a miner. On careful examination of the joint, there was a well-marked hollow over the anterior border and position of the internal semilunar cartilage. This hollow was most marked when the knee was flexed. Having decided that the case was one of displaced semilunar cartilage, and one not likely to be cured by any ordinary treatment, I, on November 16th, performed this operation. An incision was made along the upper and inner border of the tibia, parallel with the anterior margin of the internal semilunar cartilage; and, the few superficial vessels having been secured, the joint was opened. It was then seen that this semilunar cartilage was completely separated from its anterior attachments, and was displaced backwards about half an inch. The anterior edge of this cartilage was now seized by a pair of artery catch forceps, and it was drawn forwards into its natural position, and held there until three stitches of chromic catgut were passed through it and through the fascia and periosteum covering the margin of the tibia. The forceps were then withdrawn, the cartilage remaining securely stitched in position. The wound in the synovial membrane and soft textures having been closed with catgut stitches, a splint and plaster-of-Paris bandage were applied, so as to keep the joint at rest. The progress of the patient, after the operation, was perfect, the temperature never rising above 99° Fahr. Seven weeks after the operation, the splint and bandages were removed, and gentle movements of the joint practised.

On January 25th, 1884, the patient was dismissed cured, the

movements of the joint being good, and the limb steadily gaining strength. In April of the same year the patient returned to show the result. He was then seen and examined by many of our distinguished guests at the tercentenary, who all expressed the opinion that the result was everything that could be desired. He had perfect movement in the joint, and had never had the slightest stiffness or locking of the joint since he commenced to go about after the operation.

Meeting V.—March 4, 1885.

Dr H. D. Littlejohn, *President, in the Chair.*

I. Election of Ordinary Members.

The following gentlemen were elected Ordinary Members of the Society:—Allan Thomson Sloan, M.B., C.M.; John William Ballantyne, M.B., C.M.

II. Exhibition of Patients.

1. *Dr J. M. Cotterill* showed a CASE OF TALIPES EQUINO-VARUS CURED BY OSTEOTOMY. The patient, a young woman, suffered so much pain from the deformity that she was unable to follow her occupation. The sole of foot could not be put to the ground, and the tendo Achillis was very much contracted. The tibialis anticus and posticus tendons, the plantar fascia and the tendon of the adductor pollicis were first divided, and splints were put on to bring the foot into the same straight line with the leg, but no attempt was made to bend it at the ankle. A fortnight later the cuboid and external cuneiform bones were removed, along with the thickened bursa lying over the cuboid, and the tendo Achillis was divided. This allowed the foot to be brought into very fair position. The subsequent treatment, consisting in the application of the weight and pulley, effected a complete cure. The pressure made in this way was lighter than it would have been by splints and bandage, and the traction more efficient. This was a mode of treatment which had not received the attention it deserved. It could not be applied so easily in the cases of young children as in the adult, but it might be more often tried than it was. The patient could wear an ordinary boot and walk any moderate distance without trouble.

2. *Dr C. W. MacGillivray* showed a case on whom he had operated for TALIPES EQUINO-VARUS OF THE LEFT FOOT. The patient, a boy æt. 6, had come under his care about six months previously.

The deformity was very aggravated, tendons and fasciæ being tense, and the bones of the foot considerably altered in shape and position, the boy walking on the thickened integuments over the dislocated cuboid. Tenotomy of all these tendons and fasciæ was first performed, and the leg put up both in splints and plaster-of-Paris, but without any beneficial result after several months of treatment. An incision was therefore made on the outer side of the foot over the displaced cuboid, which was removed along with the anterior portion of the os calcis and the external cuneiform bone. This healed rapidly, the foot being placed on a suitable splint, and it was then found that, although the internal displacement was considerably improved, the anterior portion of the foot still continued to point downwards, through the displacement downwards, forwards and inwards, of the scaphoid on the astragalus. A fresh incision was therefore made in the position of the former one, and as the bones were so much altered that it was impossible to distinguish their anatomical relations, a truncated wedge of bone was removed, the blunt apex of which corresponded to the neck of the astragalus and scaphoid, the base to the outer aspect of the foot between the os calcis and fifth metatarsal bone. The result was to get the foot into perfect position; the wound healed without a bad symptom, and now, six weeks after the operation, the boy is walking about with perfect ease on the sole of his foot, which, although shortened and without the arch on the inner side, is perfectly movable at the level of the mesio-tarsal articulations.

III. Exhibition of Pathological Specimen and Chart.

1. *Dr E. F. Neve* showed a specimen of ANEURISM OF THE ABDOMINAL AORTA. It was taken from the body of a man, æt. 39, who had no history of syphilis, alcoholism, or strain. He had had symptoms of aneurism for nine years, during the last four of which he was under Dr Neve's care. The specimen showed that there had been first a true aneurism, that this was followed by a false aneurism in front of the aorta, hæmorrhage having occurred through some atheromatous ulcers, and that this having healed by coagulation of blood, another false aneurism of very great size had formed behind the aorta, separating it from its connexions and floating it, the first false aneurism and the pancreas against the anterior abdominal wall producing all the physical signs of a solid tumour. The lumbar vertebræ were eroded, and the patient had died of slow hæmorrhage behind the peritoneum.

2. *Dr John Haddon* showed the TEMPERATURE AND PULSE CHART IN A CASE OF CATHETER FEVER. It was a case in which he had been the nurse, Dr M'Leod of Hawick the general practitioner, and Prof. Chiene of Edinburgh the consultant, and whatever credit

surgical science had in the recovery of the case, he and Dr M'Leod were agreed that such credit must be given to Prof. Chiene. After about eight years of more or less difficulty in micturition, owing to enlarged prostate, the illness began on 10th January 1884 with great difficulty in micturition, while during the night the urine passed unconsciously. On 13th January a soft catheter was passed, and about 20 oz. of urine withdrawn. The symptoms seemed somewhat relieved, but on 19th January they became worse, and on 20th January the patient was comatose. On that day, as no soft catheter could be passed, the silver prostatic one was passed at noon, and 30 oz. of acid urine taken off, followed by a white stream of pus. Death seemed imminent all that day. At 9 P.M. the bladder was again emptied, and washed out with a solution of corrosive sublimate. At 11 P.M. consciousness returned, and he began to improve. The emptying and washing of the bladder was continued, and after much suffering he began to recover. He was dressed for the first time on 23rd April, and by the help of the catheter he continues in fair health. Sphygmographic tracings were also handed round, showing the character of the pulse throughout the attack.

IV. Exhibition of Instrument.

Dr Allan Jamieson showed a NEEDLE WITH LENS fitted on the handle, to be used in the removal of superfluous hairs by electrolysis. The wire from the battery was fixed into the handle by means of a screw, and the needle inserted, by the aid of the lens, into the root of the hair, and the circuit was completed by the patient touching a sponge connected with the other pole of the battery. A little frothing took place, and the operation was completed. The hair could then be removed by a forceps. Dr Jamieson also showed a FORCEPS with fine points to be used in the removal of such hairs.

V. Original Communications.

1. THE POSITION OF THE MEDICAL PROFESSION IN REGARD TO CERTIFICATES OF MENTAL UNSOUNDNESS AND CIVIL INCAPACITY.

By T. S. Clouston, M.D., F.R.C.P. Ed., Physician Superintendent Royal Edinburgh Asylum; Lecturer on Mental Diseases, Edinburgh University.

FROM the earliest Greek times we have evidence that physicians have been called in to treat mental diseases in all civilized countries, but it is only in comparatively recent years that our profession has, in law and practice, been made the virtual judge as to what constitutes unsoundness of mind and incapacity for civil affairs.

It does not appear that in ancient Rome any formal proceedings were required for placing an insane person under suitable treatment, but for the appointment of tutors and curators for the *furiosi* and *mente capti* there were written laws and a formal procedure. Dr Sibbald says, "Careful provision was made for the treatment of the Roman citizen when he became insane." "The Romans looked on this condition as a disease which was to be cured, if at all, by ordinary medical treatment," but, "the evidence upon which a Roman magistrate declared a person to be furious was not the opinion of experts, but such evidence as showed *that the fact was admitted by the general voice of those to whom the circumstances of the case were well known.*"[1] That statement applies as a description of legal practice in appointing a guardian to an insane person and his property for at least 1500 years. The first recorded instance we have of medical knowledge being rendered available in a formal way to enable a court of law to come to a decision in regard to the existence of mental disease was when, in 1589,[2] fourteen persons condemned to death for witchcraft appealed against the judgment to the Parliament of Paris, which named four commissioners—Pierre Pigray, the king's surgeon, and Messieurs Lerois, Renard, and Falaiseau, the king's physicians—to visit and examine these witches, and see whether they had "the mark of the devil upon them." The mode of diagnosis adopted by these gentlemen was this—"The visit was made in presence of two counsellors of the Court. The witches were all stripped naked, and the physicians examined their bodies very diligently, pricking them in all the marks they could find, to see whether they were insensible to pain, which was always considered a certain proof of guilt. They were, however, very sensible of the pricking, and some of them called out very lustily when the pins were driven into them." "We found them," continues Pigray, "to be very poor stupid people, and some of them insane. Our opinion was that they stood more in need of medicine than punishment, and so we reported to the Parliament, which, after mature counsel among all the members, ordered the poor creatures to be sent to their homes without inflicting any punishment upon them." In all respects, except their method of diagnosis, Pigray and his colleagues were to be congratulated. They stood, then, as our profession has almost uniformly done of late years, on common sense and scientific ground, disregarding the stereotyped legal views of the time, which largely reflected the popular opinions and the superstitious theories of the Church. Most of us think that this mode of formally calling to its assistance medical knowledge by the Parliament of Paris was a better method of arriving at the truth than the present British and American method of allowing each

[1] Dr Sibbald, *Morison Lectures on Insanity*, *Journal of Mental Science*, July 1877.
[2] *Ibid.*, October 1877.

side to call in its own skilled evidence as to whether a man is to be adjudicated of unsound mind or not.

In England the first Act of Parliament relating to the insane was that of 17 Edward II. in 1324, and the first in Scotland was of Robert II. in 1371. The object of both of these was to provide that the king should have the custody of the lands of idiots and insane persons during their lives, rendering the same after their death to the right heirs. But there was no provision in those Acts, or in any subsequent lunacy statutes, for medical certificates or medical evidence to prove unsoundness of mind or civil incapacity, till the English Act of 1774 for the regulation of private madhouses (14 Geo. III. c. 49) made a medical certificate, in addition to the order of a relation, requisite for the admission of every private patient to those institutions. Paupers were by that Act to be sent to such places as received them on the authority of the parish officers alone. Thus it is just 110 years ago since the adoption of that system of medical certification of unsoundness of mind, which is now universally prevalent in the three kingdoms. Every statute relating to the placing of the insane in asylums or under private care, passed since that time, makes special provision for either one or two medical certificates in the case of every patient, private or pauper.

In England and Scotland the medical certificates required by the earlier statutes were not required to embody all or any of the facts on which the opinion was founded. The English Lunacy Act of 1845 for the first time made it an essential part of a lunacy certificate that it should contain specific facts indicating insanity observed by the medical man who signs it. Other facts indicating insanity communicated by others were provided for, but were not reckoned an essential part of the certificate, and can be put in or not at the discretion of the signer. The origin of this form of medical certificate, containing the chief facts on which the opinion was founded, was the English form of affidavit usually made by a medical man, or indeed any other witness, in regard to the lunacy of a person with property to be looked after, whose state of mind was being inquired into, previous to the issuing of a writ *de lunatico inquirendo* by the Lord Chancellor. Such affidavits always contained facts proving unsoundness of mind and incapacity for the management of property, duly sworn to, and were submitted to the Lord Chancellor as *prima facie* evidence of the insanity of the party, so that he might have proper grounds for issuing the writ for a formal inquiry. This inquiry is now always made by a Master in Lunacy or one of the Lords Justices. The lunacy certificates of England and Scotland, now required before a patient is placed in an asylum or elsewhere under treatment, are in form just a compressed and simple affidavit of such unsoundness of mind as requires care and treatment. All such lunacy certificates must be in the exact form prescribed in the schedules

attached to the English Act of 1853 and the Scotch of 1857. In this absolute uniformity they differ from all the other medical certificates or affidavits required by the Lord Chancellor or Lords Justices, or by the various Acts relating to criminal lunatics or bankrupts, or those relating to persons of unsound and weak mind requiring judicial factors in Scotland.

The ancient and constitutional method of inquiring into the mental condition of a person with property, supposed to be incapable of managing it on account of unsoundness or weakness of mind, with a view of protecting that property, was and is practically the same in England and Scotland. It proceeded on the theory that the king had, as his prerogative, the custody of the lands of such persons. Before this prerogative was exercised a formal inquiry as to the civil incapacity of the supposed lunatic or idiot had to be made in England by a jury of 12 men. If he was found to be *non compos*, a person was appointed to take charge of his property, who is called in England his "Committee of the Estate," and in Scotland his Tutor. The process itself is called a Commission in England, and a Cognition in Scotland. In England this is still the ordinary process under which the means of the insane and idiotic who have property are taken charge of and used for their benefit, their persons being controlled and protected under the same process by another person, who is called the "Committee of the Person." In Scotland the ancient process of cognition has gone almost out of use. It is a curious fact, and one strikingly showing how hard it is to get modern ideas embodied in some kinds of legislation, that though in England there have been at least fourteen Acts of Parliament passed relating to commissions, and at least five in regard to cognitions in Scotland, there is not a word in one of these Acts, from the first in 1324 to the last in 1868, about calling in the skilled evidence of medical men before the decision is arrived at by the proof, or in the preliminary inquiry. And this in spite of the fact that for at least the whole of this century the actual practice has been for medical evidence to be required at every commission in England and every cognition in Scotland. The Lord Chancellor never issues a writ *de lunatico* except on medical affidavits; the Masters in Lunacy who, on behalf of the Chancellor, hold the commission, never find a person insane except on medical testimony; and no commission is ever superseded, and the patient restored to the control of his affairs, except on medical evidence. In Scotland no cognition now ever takes place without medical evidence being required. Illogically enough the Lunacy Regulation Acts of 1853 and 1862, which embody the present law of England in regard to the insane who have property, make elaborate provision for the medical visitation, supervision, and control of the Chancery lunatics after they have been adjudged to be of unsound mind.

The Scotch process of appointing a *curator bonis* or judicial factor to manage the property of those who are incapable of doing

so themselves, from mental unsoundness or mental weakness, as described in Thoms *On Judicial Factors*, is now the one in common use here. It has practically superseded the process of cognition. It can only be carried out on the production to a judge of Session, or to a sheriff if the property is small, of two medical certificates of the mental unsoundness and civil incapacity of the party for whom a curator is required, as the basis of the process. The necessity for medical evidence is a statutory provision in the appointment of a *curator bonis*, and not merely required by prescription, as in the processes of commission and cognition. The medical certificates required need not be drawn up in any set form. Like all statutory medical certificates in Scotland, they must be "on soul and conscience." The whole process is an admirable one in nineteen out of twenty cases, both for the insane and for their relatives. It is simple, implies no undue publicity, is cheap, and is easily set aside when the patient recovers. Its weak points are that it makes no provision for the custody of the person of the lunatic or idiot where such is needed, and that the process of further inquiry is a clumsy and uncertain one if opposition is made by the party or by any one on his behalf. Those defects ought, in my opinion, to be remedied in future legislation.

The English Act of 1774 recognised the scientific fact that insanity is a disease, not only by requiring the medical certificates referred to, but also by providing that five Fellows of the College of Physicians, London, should be annually elected Commissioners in Lunacy for granting licenses to private asylums in Middlesex, which licensed houses those commissioners were to visit every year. The Scotch Act of 1815 recognised the same fact, by providing that the sheriff in his prescribed visits to asylums should be accompanied by a medical man, who in Midlothian and Lanark had to be one of four resident Fellows of the College of Physicians or the Glasgow Faculty respectively. These four Fellows of these colleges had also the power of independent visitation and inspection of the asylums, apart from the sheriff, in those counties.

The same fact was much more fully and clearly recognised throughout the great English Lunacy Acts of 1845 and 1853, both of which were the work of Lord Shaftesbury, and in the Scotch Act of 1857, introduced by Lord Moncreiff, which now with their amendments regulate the treatment of the insane in the two countries. The first of these provide that three of the six Commissioners in Lunacy are to be medical men, this having been secured by the energetic action in the House of Commons of Mr Wakley of the *Lancet*, and the last (the Scotch Act) that the two paid commissioners and the two deputy-commissioners are to be of our profession. All the certificates of insanity for placing patients under treatment must, therefore, under the present law not only be signed by medical men, but when acted on, must be submitted to other medical men, who, as Commissioners in

Lunacy, have the power in England at least of saying whether they are valid or not.

All the numerous criminal lunacy Acts provide for medical certificates as to the mental condition of persons charged with crimes, or under sentence, who present symptoms of insanity, before any steps can be taken with regard to them. The English Act of 27 & 28 Vict., cap. xxix. sect. 11, in this way gives our profession virtual power in a life and death question, when it provides that if there is good reason to believe a prisoner under sentence of death is insane, the Home Secretary "*shall* appoint two or more physicians or surgeons to inquire into the insanity of such prisoner," and if they certify in writing that he is insane, the Home Secretary has no choice, but "*shall* direct by warrant under his hand" that the death sentence be not executed on the murderer, who must instead be removed to a criminal lunatic asylum. If he recovers there, the death sentence may then be carried out, but I need hardly say that no criminal lunatic was ever thus executed after his restoration to sanity.

There were in the United Kingdom 104,257 persons under certificate as insane at the beginning of 1884. For those 123,064 medical certificates had been required, or an average of about 6 certificates given by each practising member of our profession, and in each case a certificate as to mental condition has to be given and sent to the commissioners by the physicians of the institutions in which the patients are placed after their admission. The new cases of lunacy in Scotland for 1883 amounted to 2607, and as each of these required two certificates, this amounts to a yearly creation for this purpose of 5214 new lunacy certificates, or over two to each Scotch practitioner yearly. To detain a patient in a Scotch asylum after the first three years, he must again be certified by the physician at the end of every year. And every one of the patients who recover every year must be discharged by medical authority, a formal judicial procedure being required along with certificates in the case of those civilly incapacitated by commission or under curatory. To this enormous amount of medical certification we must add the medical certificates granted for the appointment of judicial factors, the affidavits for the issuing of writs *de lunatico*, those needed for the criminal lunatics, for the transference of patients from one asylum to another, for the sick and friendly societies, the more informal certificates to employers, and the casual certification for all sorts of purposes. Still further to complete the facts which show the extent of the present responsibility of our profession in regard to the unsoundness and the soundness of mind of the country, we must take into account the 500 medical men who hold official appointments as commissioners, visitors, inspectors, medical superintendents, assistant medical officers, and consulting medical officers of asylums.

Thus, within a century, have those new and most important

duties and high responsibilities been gradually devolved by the Legislature and by judicial procedure on our profession in regard to mental unsoundness and its resulting incapacity to manage property. We have become the virtual judges of every question in regard to insanity, quite apart from and above its treatment as a disease. We control the liberty of the subject *quoad* insanity. It is through us alone that a man can be deprived of all his civil rights and position, the enjoyment of his property, the society of his wife, the control of his children on this account, and through us he is restored back to his position in life again. Our power is none the less real nor our responsibilities less heavy that the technical "order" or "decision" for a man's being deprived of liberty or civil rights is in Scotland one given by a magistrate. If the magistrate cannot act except on our certificates, and cannot virtually refuse to act if our certificates are valid and uncontradicted, we are the real power at his back. As a matter of fact, the medical profession have not sought those great powers in regard to unsoundness of mind. Many distinguished men have strongly set themselves to stem the tendency to give us such power. Lord Westbury, reckoned one of the ablest judges of his time, said, when Lord Chancellor, that "the introduction of medical opinions and medical theories into this subject has proceeded upon the vicious principle of considering insanity as a disease; whereas the law regards it as a fact, which can be ascertained in like manner as any other fact." Such protests have been utterly unavailing to affect recent legislation or procedure. Scientific fact has carried the day.

I am not aware that our authorized representatives were even consulted when the Legislature at those various times devolved on us these duties. I shall presently allude to the question whether we can evade or decline them. As it is, we as a profession have up to this time virtually accepted them. I am not at all sure, however, whether we have fully realized our position in regard to this matter. I doubt whether we have looked the thing fairly in the face, satisfying ourselves as to the principles of our position and our action, weighing our responsibilities, and calculating the risks we run. There are times when, in regard to such important questions, principles have to be inquired into, new departures made, and standing ground looked to. We are in the position of a man who has, without asking for it, gradually got into his hands the management of a whole department of State. While things worked smoothly and in a routine way he never asked himself any questions about why he came to be there. He simply did his duty to the best of his knowledge and ability. But when a crisis came, and things began to go wrong, and people said to him, "What business have you to do what you are doing?" he, if a wise man, sets himself to consider his position. It seems to me the medical profession has just now come to such a halting and considering time in its medico-legal position with regard to certifying insanity.

Judges from the bench are describing the present lunacy laws and procedure as "shocking" and full of danger to the liberty of the subject. Juries are mulcting medical men heavily in damages for signing statutory certificates of insanity. The press cries out that something is wrong, uttering discordant and contradictory views, but still showing an uneasy feeling. The medical papers assure the profession that things are wrong, and must be put right. Questions and debates in both Houses of Parliament exhibit the combined ignorance and alarm of our legislators, and finally the Government promises a bill to put things to rights.

I hold most strongly that in regard to certain aspects of future lunacy legislation the medical profession is entitled to have a direct and controlling voice. That would simply be recognising the scientific fact that it is a disease. Though in Scotland we are not so dissatisfied with our lunacy laws and lunacy arrangements as they are in the South, and have not been subject to such periodic lunacy scares as have occurred there, especially among London journalists hard up for topics that will excite the public, yet it behoves us carefully to consider the matter, and know our own minds in regard to it. If legislation is carried out for England, the same principles will soon no doubt be applied here. If we are wise, and mean to make our influence felt, it will not do for our medical corporations and our universities to show apathy or ignorance when a bill affecting a widespread disease, and the relationship of the profession to that disease, is passing through Parliament. To speak plainly, the lawyers have been able to shape former lunacy bills too much according to their views of mental unsoundness and civil incapacity, through the apathy of medicine. We hold the key of the position if we have the wit to use it. There are other bills than those about medical reform, infectious diseases, and vivisection, that we ought to be interested in. That disease of the brain, whose chief manifestation is derangement of man's reason, is surely one of these.

If the position of the profession in relation to mental disease has grown so vastly of late years, there can be no reason to question that it will continue to grow in the future. The advance of cerebral physiology and pathology is leading to advances in the early treatment of mental diseases by the family practitioner. There was a time when the mild forms of the disease were scarcely treated at all. As we all know, such treatment requires certain special arrangements that the treatment of no other disease requires. Mental nursing necessarily implies restrictions on the patient's power of action. It often implies the use of force to overcome resistance. To be effectual it constantly implies removal of the patient from his own house against his will. It may imply the forcible administration of food or medicines. To cure the disease, in short, the doctor needs to control the whole circumstances of the patient, irrespective of the patient's will. I am not now referring

to the sending of patients to asylums and the treatment that may be adopted there, but to home and private house treatment by family physicians under the sanction of relations. Now, in regard to all this the profession is entitled to take the broad ground, that whatever is needed for a patient medically for his cure should be legal and unrestricted by any vexatious statutory provisions whatever, and should imply no undue risk to the doctor. We are already much better off in this respect in Scotland than they are in England, in having a provision of our law that for six months, with a view to recovery, a patient may be treated anywhere the doctor and the relatives think best, without any legal formality or word of technical insanity whatever, except a private letter of intimation to the Commissioners, if he be treated from home and his board paid for.[1] If treated at home, or in a house or lodgings paid for by him or his friends, no such intimation even is needed till after twelve months.[2] We must see that our present Scotch liberties in this respect are not interfered with by future legislation.

When a patient has to be formally committed for treatment to a hospital for the insane, his personal liberty is necessarily interfered with by strangers not acting like a private nurse under the direct orders of his relations. Therefore it seems to me that for this some such system of formal medical certificates as the present must be continued. The only question is—Who is to give them? I am in favour of the present system of the whole profession retaining the power of granting such certificates instead of deputing it to special Government-paid officials. I do not even approve of one of the two certificates being granted by such an official; for if the family doctor had granted the first certificate in any case, and the Government official refused to grant the second, the former would be in a very unpleasant and dangerous position indeed. I contend, however, that the medical man who certifies a case of insanity should be in exactly the same position as a witness who has been summoned to a Court and gives evidence there in answer to questions asked. If he does not say what he knows to be untrue, and if he has no *mala fides*, the witness is protected by the Court from any consequences that may arise from his testimony. So should the doctor be in giving a truthful lunacy certificate.

Then arises the question—If two medical certificates in statutory form are given in regard to any case, is the sheriff to judge of the sufficiency of the facts indicating insanity contained in them, and grant or refuse the "Order" to receive the patient into the asylum, according as he thinks those facts sufficient or insufficient? At present different sheriffs act on different principles in regard to this vital matter. One acts judicially, another ministerially. I have known a sheriff refuse to grant an order because it was stated in one of the certificates (both of which contained abundant facts indicating insanity) that the woman had been confined the

[1] 20 & 21 Vict., cap. lxxi. sect. 41. [2] *Ibid.*, sect. 43.

day before. Being a kind-hearted man, he thought it shocking to send any woman to an asylum the day after her confinement; and being the judge in the case, and there being no appeal, though there was imminent risk of the insane mother strangling her baby and jumping out of the window with not a soul to look after her, those risks had to be run. And I have received an order from another sheriff for the admission of a patient, when one of the certificates under "Facts indicating insanity observed by myself," stated, "I could discover no fact indicating insanity." There being an express clause in the Act absolutely prohibiting any patient being received into an asylum except on certificates containing facts indicating insanity observed by the certifier, I considered I could not admit the patient, notwithstanding the sheriff's order, and refused to do so. It is notorious that sheriffs, or rather the sheriff-clerks, do not see that the formal legal portion in the first parts of the certificate before "Facts indicating insanity observed by myself" is correctly filled in, which is just the part they ought specially to see to, in my opinion. In England a certificate is not valid except the certifier's exact medical qualification is put in. This is constantly omitted here, and yet the sheriffs sign the order. There is a formal English decision that if the patient is not designated and his residence put in, the certificate is invalid and worthless. It is quite the exception for me to get a certificate where the patient is designated, except one signed by an old student of mine, who had remembered my drillings on this point.

There are two opinions given and certified to in every certificate. One is that the patient is "a lunatic," or 'an insane person," or "an idiot," or "a person of unsound mind" (and, by the way, those expressions become in this way the statutory definition of legal lunacy); the other is that the patient is "a proper person to be detained under care and treatment." Both things should be taken into account by the doctor, each separately and distinctly from the other, before he signs. If we grant that the sheriff can judge of the sufficiency of the facts to prove the first, that of "unsoundness of mind," it is quite certain he is not in a position to judge as to second, viz., whether the patient is "a proper person to be detained under care and treatment." That is a medical question, which a medical man alone can solve, and one in which the law gives him no help whatever.

If one is to adopt the method of interpreting the meaning of a document from its contents alone, then the sheriff's order bears no proof within it that the sheriff has considered the facts in the certificates, or is satisfied with them. It simply says that "having had produced" to him a petition and certificates, he authorizes the transmission and reception of the patient.

I therefore conclude that the sheriff does not act judicially in granting orders for the reception of patients into asylums. He is merely bound to see that a petition, statement, and medical cer-

tificates are there in due form, and accurately filled in. His position ought to be defined in the next bill we have. I do not think he is properly qualified to say whether the "facts indicating insanity" do indicate it or not, that being a medical question, just as is the question of suitability for care and treatment. But what is the good of the sheriff's order in that case? It is "a mere farce," say some of our English brethren, "pretending to be what it is not, and taking the proper responsibility off the petitioner and medical men." I dissent from this very emphatically. I look on the provision for inserting "facts indicating insanity" as one affecting the doctor, to compel him to formulate the grounds for his opinion, and not one with which the sheriff has judicial concern. Before the present Scotch Act came into operation the medical certificates contained no "facts;" and yet, on the medical opinion that the patients were insane and should be admitted into asylums, the sheriffs granted orders for that purpose. No "facts" are required in the medical certificates for a man being deprived of the control of his property and a *curator bonis* appointed for him by a Lord of Session, and no "facts" are stated when the Secretary of State respites a man from the death sentence on account of insanity. The sheriff's order is most important, apart from its being a judicial decision as to the insanity of the patient. It secures that personal liberty is not interfered with *quoad* lunacy, except on a magistrate's authority, that the papers are in statutory form, that everything is *bona fide*, and that the medical men who sign are on the Medical Register. It secures that amount of publicity in the whole transaction which is needed to give the public confidence in the working of the laws.

But while that is my opinion in regard to the sheriff's non-competency to review and judge as to the "facts" stated in the medical certificates, I am not to be taken as implying that therefore those facts are unimportant, or should be carelessly dealt with by men in signing their lunacy certificates. On the contrary, I would put them on higher ground than that they should be merely sufficient to "pass the sheriff." I would urge on my professional brethren that they should be sufficient to satisfy the more accurate demands of science. I would say that each case should be so carefully examined into, and the reasons for coming to the decision to certify the patient so clearly arrived at in one's own mind, that the writing down of the "facts" would be a mere formulation of the most important reasons for the conclusion already come to. "Is the patient insane?" "What evidence is there?" "What kind of insanity does he labour under?" "Is he a proper person to be detained under care and treatment?" "Why should he be so detained?" If one is able to answer these questions clearly in one's own mind, there is usually little difficulty in filling in the "facts." I would say that the chief things which require asylum treatment and imply medical certificates are danger to the patient or others

without proper means of obviating such danger, disturbance of the public peace, offences against public decency, the necessity for care and treatment which cannot otherwise be got, the presence of such symptoms as can only be properly treated in a hospital for the insane, the failure of treatment otherwise, and the deliberate medical opinion that hospital treatment is best for the patient. As we all know, all sorts of considerations—social, monetary, and domestic—must be taken into account in deciding the question.

When the certificate is being prepared, we certainly cannot do so too carefully: one should look at the directions on the margin in each case, and read over every certificate before it leaves one's hands. To get "facts" to put down, one should first think of the marked insane delusions, then of incoherence of speech, then of suicidal or dangerous occurrences, then of shouting or outrageous conduct, then of incapacity to understand questions or to answer them sensibly, then of insane reasons assigned for conduct, then of changes from the natural character and conduct, then of abnormalities of appearance, facial expression or manner, then of negative signs, such as taciturnity and insensibility to impressions. Quote the patient's words, if possible. If we put our facts down in a pithy, dogmatic way, using definite but non-professional expressions, and if the process of diagnosis with subsequent certification is founded on a fair knowledge of mental disease, I do not think we run much risk in signing certificates even in the present state of the law in Scotland.

I ought to have premised that before any medical certificate of insanity is signed under any circumstances, the necessity for it must be fully recognised by the patient's relations. They must, in fact, ask us to certify. In this a certificate differs from a medical prescription essentially in principle. After we are called in to a case, we do not ask whether we shall give strychnine or not, if we think the patient needs it. There are few circumstances where I should even press the relations of an insane patient to come to a conclusion about having him certified. They should be fully satisfied, and should initiate any action taken. Our duty is to state the risks involved, legal and social, the benefits to be derived from treatment under certificates, and our medical reasons so far as we can explain them. The position of a medical man in thus being requested and authorized to sign a medical certificate by a responsible relative or public authority undoubtedly entitles him to exemption from the risks that he would run if he initiated the process. He only, in fact, makes a formal statement of a medical opinion, founded on medical observation, and his reasons for it.

And now we come to the consideration of the question—What are the risks to a medical man who signs a good medical certificate of insanity after the sheriff has granted the order? There are risks undoubtedly, but not many in Scotland. Since our Lunacy Act of 1857 passed, there have been over 46,000 certificates signed,

and as yet no medical man has been brought into Court for a certificate of lunacy. Considering that any man in this country may bring an action against any other man for almost anything real or imaginary, this immunity on the part of our profession in regard to a proceeding of such importance, and one which is apt to cause such irritation as the making a man legally into "a lunatic," is very remarkable. But can we avoid any risk whatever? I cannot see how we can do so. There are a few difficult cases where the necessity for certification may be so great, and yet the risk so apparent, that it is quite right to ask for a letter of indemnification against pecuniary loss from legal proceedings from the person who asks for the certificate. But I confess this is a proceeding that does not look well on the face of it, for it seems as if we were doubtful about the propriety of granting the certificate, and I only advise it in very exceptional and rare cases. In the case of pauper patients, it could scarcely be got. Yet an action brought against a parochial medical officer, who receives no special remuneration for his medical certificates, and no protection from the parish against loss from legal proceedings, is a possibility, and would be an especially hard thing. I am of opinion that this duty is of so responsible and onerous a nature that its remuneration should never be included in ordinary parochial work, but that a special fixed salary should be attached to it. A perfunctory and careless mode of doing it is encouraged, and its importance not realized, if it is not paid for. I am sorry to say that the parochial boards of our larger towns are especially pennywise in this respect. It is not so in England. I think our General Lunacy Board and our medical corporations should make a representation to the Board of Supervision on this point.

It is in the case of the patients whose insanity is obscure, those whose power of self-control is great, those with only partial insanity, those whose insanity consists in foolish actions while their reasoning power is acute and their speech coherent, those in whom the mental unsoundness consists mostly in moral perversion, and those who labour under that form of insanity from drinking in which all the symptoms pass off quickly after being put under treatment,—it is signing certificates in the case of such persons where special precautions have to be taken and special risk is run. There are some people of means, too, whose dispositions have been so litigious, that they need to be very insane before any man will venture to sign lunacy certificates for them, and there are others with semi-insane suspicious relations in the same position. To meet exceptional cases, and place such persons under treatment, to save them from ruin, and to save their relations from manifold annoyances, I think there should be provided a formal process, by trial before a Court, similar to the process of cognition, but applicable for the object of placing patients under treatment, cognition being only applicable at present where there is property

to protect. Such a process would only be making lunacy laws conformable to the facts of disease. It is in some such cases that we, as a profession, are at present justified in absolutely declining to sign the ordinary statutory certificates. If such patients are dangerous or very threatening, the present Scotch procedure for placing dangerous lunatics under custody at the instance of the procurator-fiscal is the proper remedy. Many a suffering wife and relation, whose lives are constantly in danger from alcoholic maniacs, would do wisely to resort to this. The question of the patient's mental unsoundness and dangerous tendencies is then tried in open Court according to law. And no one runs any risk, or incurs any responsibility, but simply gives evidence in Court when called on to do so. I have had several patients under my care, where endless trouble was saved through their having been "fiscal cases." The publicity of such proceedings is disagreeable at the time, but it may save from worse things.

It has been proposed that very soon after a patient is sent into an asylum some Government official should see him, examine the papers, and if satisfied, should sign an "Order" which would then be the permanent authority for his detention, thus taking all responsibility and risk after that off the persons who placed him there. I cannot see that this would be any improvement on the present procedure, which consists in the physician of the asylum sending a certificate to the Commissioners in Lunacy with the form of mental disorder within a week after admission in England, and within a fortnight in Scotland, and the subsequent visits of the Commissioners. The asylum physician thereby homologates the certificates and shares the risks of legal procedure, the cost of which in his case would be paid by the institution if he acted legally and *bona fide*. It is better to have an institution sued than an individual. No examination by an official would exempt from the risk of action for sending a patient to the asylum; and if by possibility the proposed official, not being able in the course of his one visit to make out the patient's insanity, and not having, like the physician to the asylum, who gives the certificate at present, the opportunity of seeing him on admission and every day after admission, if he should not be able to give his *imprimatur*, then the risk to the certifying medical men would be great indeed, for they would be virtually convicted of a wrong diagnosis by the man appointed to protect them from risk. At present no action can be brought against a medical man in Scotland for a certificate after twelve months from the time the patient has left the asylum,[1] so that the prolonged annoyance and expenses suffered by the medical men in the MacIntosh *v.* Smith and Lowe case could not occur under the present statutes.

If I had the shaping of future legislation, I should institute a provision that under certain exceptional circumstances even patients

[1] 29 & 31 Vict. cap. li. sect. 24.

in asylums, who could show good cause to the Commissioners in Lunacy, should have the chance of a public formal inquiry for settling the question of their mental condition in a definite way once for all. We have patients who, having laboured under acute symptoms on admission, become partially recovered chronic grumblers, with the grievance that they are confined because they cannot get a fair open hearing or examination. Such a provision would satisfy that inherent sense of fair-play and justice which is seldom lost in the insane; and it would also satisfy some of the public better even than the Commissioners' visitation or calling in two independent medical men, as provided for by the Scotch law. The publicity involved in such inquiries is no greater than that involved in being insane, for whose friends and neighbours do not know all about a man's mental troubles? And if it were somewhat greater, it should be faced to avoid still worse evils. It is an enormous gain to make all lunacy proceedings above board, and to correct by law the natural but false shame that tends to concealment of everything connected with unsoundness of mind in a family. Our sheriffs' orders have done us much good in Scotland in that respect.

A very strong position in regard to unsoundness of mind has for some time been taken up by the *Lancet*, the oldest and not the least influential of the weekly medical press. It is shortly this. "Our lunacy laws are utterly and radically bad. Our lunacy policy is wrong. The Commissioners in Lunacy are very inefficient. The position of medical men with regard to lunacy, and the present lunacy certificates for admission to asylums, is untenable. Asylums are conducted on wrong principles. Let the profession sign no medical certificates of insanity whatever under any circumstances. Let them treat lunacy as bodily diseases are treated by doctors, without reference to any medico-legal aspects of it. Let the State look after this. Don't submit to the indignity of submitting medical certificates to a magistrate."

This to many of us seems an impracticable position for a responsible organ of medical opinion to take up, even supposing the present lunacy laws are not what they should be In the first place, the British public would have good right to say to the medical profession, if it acted on the advice of the *Lancet*, "You have no moral right to inflict on society the inconveniences and dangers that would result from a sudden stoppage of the working of the lunacy laws, and the gigantic lunacy arrangements of this country that you have helped to build up and assisted in carrying out for forty years. You have no right to put individuals to the annoyances that such action would cause. You, who owe your professional privileges to the law, are morally bound to carry out the present law while it exists." I tried by a letter to evoke from the *Lancet* a formulated statement of what it would propose instead of the present laws and arrangements, but failed. Its tone

is a mixture of bitterness, disloyalty, and conscious "crying in the wilderness" on this matter, though I sympathize with its efforts to strengthen the purely medical position in regard to insanity. I cannot help feeling that it is unjust towards the lunacy laws, towards the commissioners, towards asylums, towards alienists, and, above all, towards that grand philanthropic movement to benefit the insane embodied in Lord Shaftesbury's labours in that cause which has on the whole had such great results. Would any physician who had been for years the trusted adviser of a family, be morally justified in refusing to certify, when requested to do so, a member of it who had become suddenly acutely maniacal, and for whom the means of treatment did not exist at home, because the lunacy laws were bad and because he ran some personal risk? Do we not all run risks every day in other ways? Would the family be able to distinguish the doctor's conduct if he so refused from that of a man who would not attend a case of typhus in the family for fear of infection? No doubt there is a strong feeling now in England, and especially in London, against signing certificates. Many medical men there will not do so under any circumstances. I heard the other day of an undoubtedly insane man, whose relations had gone to forty doctors in London, and they all had refused to sign a certificate for his being sent to an asylum. I think this feeling will not subside for a time in regard to doubtful cases, but I have never heard of any real difficulty in getting certificates for obvious cases here in Scotland. And I don't think there ever will be any difficulty in such cases so long as the certificates can be paid for.

I cannot refer to one or two important trials that have lately occurred in London, because the decisions come to have been appealed against to higher courts, and they are therefore *sub judice*, but I can refer to what some of the English judges have lately said. Lord Coleridge some time ago expressed astonishment that the two medical certificate forms were on one sheet of paper, implying that the signer of the second certificate could see what the first contained. No doubt it is stated in the statutory form that each medical man must have seen the patient "separately from any other medical practitioner," but as a profession we never sanctioned that proviso, which it would be far more rational and better for the patient to reverse, and put "with another," for "separately from any other." I don't know any sort of case where a talk with a professional brother is more useful than before coming to the conclusion that a patient should be sent to an asylum. There is then every reason that exists for an ordinary consultation, plus special risks to the patient and to both doctors. All the medical reasons go in the teeth of the remarks of the Lord Chief Justice. His blame did not touch our consciences.

Baron Huddleston has lately presided at two lunacy trials, and in one he made some strong remarks about some of the present English

proceedings being "shocking," and about the especial danger involved in that provision of the English Acts which admits of any one ("any pauper," as he said), not being a relative of the patient, or even connected with him in any way, signing the order of admission, which in England takes the place of sheriff's order here. I am of opinion that in England such "orders" of private persons will have to give way to a magistrate's "order." But the principle involved in Baron Huddleston's criticism applies to the question of who is to be the "petitioner" in our Scotch form. I always advise that no stranger, or other person not having a real relationship to the patient, should be the "petitioner." If there is no relative or guardian to be got, then let the inspector of poor, as the most convenient public official, be called in and act. Temporary arrangements can be made in any asylum for any patient being treated as a private patient, though technically a pauper. It is a bad principle for a hotel or lodging-house keeper, or other person with a mere casual relation to the patient, to be the petitioner. More lately, the learned Baron held that merely looking at a man through a carriage window was not a statutory "examination" of him by two justices, and this seems reasonable. It is most important for us as a profession to see ourselves as others see us, and whether we agree with them or not, to consider most attentively what the judges who administer the laws under which we act say about us and them. We must remember that medical and scientific ideas about disturbance of the mental functions of the brain must necessarily be only partially intelligible to lawyers and to the public, and *vice versa*, as Sir James Stephen has strongly emphasised.[1] It seems to me, in reading lunacy trials, that their very rarity makes the judges inexpert with regard to them. It sounds ludicrous to hear a judge express astonishment at the existence and danger of statutory provisions that have existed for forty years, and have worked fairly well all that time.

Everything that has taken place in recent lunacy trials should impress on us as a profession the necessity of acting with deliberation in regard to lunacy certificates, of taking no more than our fair share of responsibility, of consciously thinking of the risks we run, of keeping within the letter and spirit of the law stringently in the minutest particular, and of declining to act when there are good grounds for doing so. We should go as little beyond our professional work, too, as possible. In the present temper of the public and of the judicial mind, it is better to be able to justify all we do on grounds of professional necessity. I do not think we can reasonably expect to be protected by the law against all risk in lunacy practice any more than in surgery or general medicine. All we can ask is that future legislation in regard to our relationship to mental unsoundness shall fully recognise it to be a disease which science and public necessity have

[1] *History of the Criminal Law of England.*

made us the sole judges of. If the laws, the general public, or the judges, set up tests and definitions of insanity opposed to those of science, then we shall be justified in handing over the whole subject to them, to see what they can make of it, and ourselves declining to act as certifiers. I cannot say I see many signs of this. There is much talk, but few signs of action. We should, I admit, have some reasons for considering our position, if frivolous actions like those brought against the physicians to Bethlem Hospital and two other London practitioners, the other day, became common. In that case we should be justified in asking that actions in regard to lunacy certificates should be put into the category of criminal prosecutions, only to be undertaken by the public prosecutor, or by his leave, and not by private parties in civil suits.

Instead of lessening, our duties in regard to disorders of the mental functions of the brain will be likely to grow apace as time goes on and our knowledge increases. The area covered by the disease will increase, let the public protest as they will. Its beginnings and lesser degrees will be more and more recognised to be a factor in conduct and motive. It will be more clearly seen that "the will" is tempered by brain action, and that a man is often overmastered by the "tyranny of his organization." We shall be asked, and are being asked, more and more to examine persons with the view of giving formal opinions as to their testamentary capacity before wills are made. Instead of having a man, in a doubtful state of mind, make a will, and then after his death have all his money squandered in a costly lawsuit to determine whether he was sane when he made it, we shall be asked to examine him and make a formal report of his testamentary capacity, which, with our grounds for the opinion, will virtually decide the whole question. Those who have a *bona fide* interest in the destination of a man's property after his death should have the right by law to have such a medical examination and such a report, if there is *prima facie* evidence of doubtful sanity or impaired mental capacity on his part, irrespective of the sanction of the testator. Few things in my professional life have given me more satisfaction than some examinations and reports of this kind that I have had to make, which have had the effect of doing justice and upsetting nefarious or improper influences that were being exercised to make men in weak or unsound mental conditions leave their means to the wrong persons. The first case of the kind I ever had to do with made a deep impression on my mind, where a report of my late friend Dr Elliot of Carlisle and myself, on the testamentary incapacity of a Cumberland statesman, had the effect, without any trial in a court of law, of squashing an improper will made, or about to be made, at the instigation of a person in attendance on him. I set myself to prepare a series of tests and medical matters to be in-

quired into in examining a testator in such circumstances, which my subsequent experience has enlarged, and which I have found of the greatest service. I once heard the late Sir John Holker say that he had never known, in his vast experience, a will upset, if made after a medical man had examined the testator as to his mental capacity, and if a respectable agent had drawn up the document, neither of them taking any benefit under its provisions. Why should we have to go into witness-boxes, after having been carefully primed with the facts bearing on one side of the case only, to give mere opinions on imaginary cases, which the other side easily neutralize by facts unknown to us before, when we might have examined into the whole facts during the testator's lifetime? I anticipate the time when no respectable agent will consent to draw out a will for any man about whom there can be the slightest doubt as to his full mental capacity, or even to draw out a will containing strange, unnatural provisions for any man, without a formal medical examination and report on his mental condition.

I believe, too, we shall be asked to report on and certify as to the mental condition of persons about to transact important business, to enter into engagements to marry, to assume important offices and positions in life, where there are circumstances in such persons' lives or history pointing to doubtful mental stability, or even where there are strong hereditary tendencies to mental disease, far more than we have hitherto been. There is no doubt of this, that if we had such formal examinations and reports to make, we should become better medical psychologists, and study hereditary laws far more than we do now. A great advance would soon take place in our knowledge of cerebral mental function, and scientific conclusions would take the place of much educated guessing.

In regard to the question of the curability or incurability of any case of mental unsoundness, on which the greatest issues constantly depend, we shall no doubt be called on in future more frequently to give opinions for various purposes, and have to annex our reasons. The tenure of appointments, the receipt of pensions, the investment of money, the future arrangements and residence of families, the education of children, may all depend on medical opinion as to the curability of certain cases of insanity. I know a case now where the whole management of an estate is brought to a standstill because I can't say effectually that the owner is incurable. If the law would give force to my opinion, by a specific provision that it was to be taken in such a case, the agent could act more freely for the benefit of the patient and his family. I have another case where, if a certificate of causation and incurability from me had any legal sanction, a man would get a good pension, instead of running the risk of being dismissed from his office. There is a very large question of lunacy policy and expenditure of public

money, the solution of which depends on the curability or incurability of pauper patients. Other large cities will have to follow the example of London, and build cheaper institutions for the incurable portion of their pauper lunatics, their idiots and imbeciles, instead of making never-ending extensions of the present more costly asylums, in the vain hope of thus providing for such persons. To remain in a certain kind of asylum, therefore, will probably in the future depend on a medical certificate of curability, or, at all events, of being capable of receiving benefit from medical treatment, while the entrance to the other kind of institution will depend on a medical certificate of incurability; and this will need the sanction of new legislation.

I have no time in this paper to enter on the question of responsibility for crime on account of mental disease, and the general view of the medical profession as to the present unsatisfactory mode in which medical opinions and knowledge are brought out in Court. We shall never get out of the present most unedifying and sometimes scandalous partizanship and tendency to contradict each other in the witness-box till we are appointed by the Court impartially to examine the prisoners, with access to all the facts on both sides, and make a formal report to the judge.

There is a great social question looming in the future, and already a practical one in some of the American states, and I believe in one or two countries of Europe, in which our reports in regard to the curability or incurability of mental disease may determine whether there are or are not grounds for divorce. Many publicists of weight have expressed the opinion that absolutely incurable insanity in either of the parties should be a valid ground of divorce if the other party desires it.

To be able to do the duties required of them under the lunacy statutes, to give opinions on civil incapacity, to guide individuals and families in regard to those momentous questions I have spoken of, to give their patients the benefit of the latest scientific knowledge in treating mental diseases, medical men now entering the profession should be required to show some practical acquaintance with this department of medicine. The Royal University of Ireland has taken the initiative in this matter. It cannot be that the other medical licensing bodies will lag far behind. If our profession had been as well instructed in insanity and lunacy law as they are in midwifery there would have been fewer lunacy lawsuits. It is a matter of much regret that the General Council of Medical Education has not taken this important question up.

An improvement in the lunacy laws will not alone be sufficient to allay the public distrust, for that partly rests on ignorance, and public enlightenment will only follow the enlightenment of the profession in this matter.

The President, in congratulating Dr Clouston on his paper, said

he had received apologies for absence from several gentlemen, including Dr Rutherford of Dumfries, Dr Campbell of Carlisle, Dr Howden of Montrose (who said that had he been present "he should have argued that when medical men were called in to certify they should be paid for their opinion rather than for their certificate"), the Dean of the Faculty, Mr Guthrie, Mr Gibson, and Mr Chas. Scott, advocates.

Dr Ireland thought Dr Clouston had covered so much ground that any one rising to speak would, if he tried to do justice to all the points the writer's sagacious mind had raised, have to make a speech as long as the paper itself. He felt that he could only touch on one point, and that was the position of the sheriff in relation to medical certificates. He could not agree with Dr Clouston that the sheriff's duties should be merely ministerial; that he should simply receive the opinion of medical men and act on it, without allowing his own judgment to have play in the matter. Certainly one good point was made against the legal functionary, and that was whether he was capable of knowing if an asylum were a proper place for a lunatic or not. He (Dr Ireland) must think that the power assumed by the sheriff was a great advantage to the medical profession. Had a similar power existed in England in committing lunatics to private asylums, they might have heard less of recent cases than they did, and the public distrust would not have been aroused. No doubt the sensational articles that had been written had much to do with this feeling of distrust, but if there were a stipendiary magistrate to look over certificates, the distrust would be greatly lessened. For his own part, rather than that the sheriff should be deprived of his power, he would have him study a little insanity, or brain physiology.

Dr Campbell said there were in the paper several points of interest to the general practitioner. One of these was the suggestion that representation should be made to the Board of Supervision that the salaries of parochial medical officers should not include their fees for certifying lunatics. The Board of Supervision held that the salaries and fees should be separate, but, unfortunately, medical officers were compelled to submit to the pleasure of the country Boards. Another point of importance was that the fact setting forth the phase of insanity might be, in the smaller asylums, at least, of some value, as indicating the mode of treatment that should be adopted.

Dr Yellowlees of Gartnavel had noted one or two points of special interest in this very valuable paper. The most significant, and one of very eminent importance, was the position of the sheriff in relation to the medical certificates. Was his granting the warrant a judicial or a ministerial act? It was within his knowledge that in the same county sheriffs took different views on this point. Some sheriffs criticised the certificates with very great care, and refused

the warrant if they deemed them inconclusive. Others, on being satisfied that they were formally correct, granted the warrant forthwith. He did not quite agree with Dr Clouston as to the wisdom of regarding the sheriff's act as purely ministerial. He thought the granting of the warrant entitled the medical men to protection, and it should therefore be considered a judicial act. In the certificate there were two distinct things: first, the facts setting forth the insanity; and, second, an opinion founded on these facts that the person was of unsound mind, and should be detained in an asylum. The sheriff might or might not be able to make very much of these facts, but they were set forth for his consideration, his warrant gave effect to the opinion founded on them, and whether his signature protected or not, it ought to do so. He agreed with Dr Clouston as to the importance of holding public inquiries into the state of lunatics who were troublesome or dangerous outside asylums. He did not think the procurator-fiscal was called in as often as he should be. There were cases where the friends would not interfere and would not call in medical men, and where the medical attendant could not interfere, but where the fiscal could, and with good effect. Unfortunately, the fiscal refused to take cognizance of any act done by a lunatic, unless it was done in public. He did not agree as to the wisdom of granting any and every troublesome patient in an asylum a public inquiry whenever he chose to demand it. It would be giving such a patient too much importance to take him before a public court for inquiry. In the Commissioners of Lunacy they had all that was needed for such cases. Some patients would complain though fifty courts found them insane. He was afraid that an action for damages, possible only at the instance or with the consent of the public prosecutor, might prove to act very hardly and unfairly on medical men. He did not think that would be a wise way of averting the troubles now experienced by their brethren in the South. Testifying to soundness of mind was, he thought, a duty they would be more frequently called on to discharge in future. It was a point of much importance in relation to many transactions, such as making wills, important business engagements, etc.; and it would be found a far more delicate and difficult task to testify to soundness of mind than to certify unsoundness. Nothing could be more unwise than to grant a certificate of sanity to a stranger after a brief interview. He had known a distinguished practitioner incautiously grant such a certificate to an undoubted lunatic who had just escaped from an asylum. He considered Dr Clouston's remarks on this point of extreme value.

Sheriff Thoms, on the call of the President, made a few remarks. He was afraid the Society might consider that his soundness of mind required to be testified to before he spoke to them on one or two points in Dr Clouston's paper. He believed that every one was cracked in some respects on one point or another. The legal

aspect of insanity being one of his "fads," he was afraid he might be in danger of being considered unsound on this question. He was, however, sound enough to appreciate the value of the paper. On one or two points he did not agree with Dr Clouston. He (Dr Clouston) rather ignored the historical aspect of the law in Scotland in reference to lunatics not possessed of property. It was only of late years that punishment for crime followed where there was no property possessed by the accused. No one was punished unless he had property till about 150 years ago, and could pay not only a fine, but a sum into the "fisk." Sheriffs, procurators-fiscal, and sheriff-clerks had no salary until 1747, and had to be paid out of the Court's treasury or fisk (the Renfrewshire court-records were the most accessible evidence on the point, as they had been printed and published), and if a man was a beggar, he was not prosecuted, as nothing was to be got from him for the fisk. Cases of lunacy were similarly treated until lately, and it was those in which property was not involved in which the inspector of poor was called on to act. There were various influences at work here. Dr Yellowlees had said that proceedings at the procurator-fiscal's instance would be a help in cases where the friends would not, and the medical profession could not interfere; but when cases came to the fiscal the county had to bear the expenses, and the fiscal's anxiety was to get the thing done as cheaply as possible. He therefore went to the inspector of poor, because, if the fiscal could not recover from the county, the inspector had to pay. This matter of payment had a great deal to do with the question. The lunacy grant, in his counties at any rate, had had a very deteriorating effect on the inspectors of poor and their anxiety to increase the class of lunatic paupers, and he should be sorry to make his substitutes in the North believe that their duties under the Lunacy Act were ministerial rather than judicial. Many cases were brought before them rather to get the lunacy grant than to have the insanity declared. He therefore himself scanned every certificate he got, and he should not like that the affixing of his signature should be considered as merely a ministerial act. It was of the utmost importance to have the matter viewed in a more serious aspect. It was very different in other places where the medical profession occupied such a position as, say, in Edinburgh or Glasgow; and this was what he felt, as regards the legislation of Sheriff Courts,—that our legislators and those who inspired their action were too much inclined to consider places in the far North and in the remote West as on the same platform with Glasgow, Aberdeen, Edinburgh, and Dundee. What would do for large towns would not work in remote localities. The position of inspectors of poor in the North was such as almost to be a disgrace. He had to try one inspector lately for culpable neglect of a pauper woman. The inspector had given her no

attendance, save that of an old sailor, to dress sores on her thighs, the result of dropsy. His conduct received the imprimatur of the local board. She died. They could not try the inspector on a charge of culpable homicide as he deserved, as the woman's body was so covered with vermin that no post-mortem examination could be made. But he was tried for culpable neglect. It was a difficult matter to get a conviction, most of the jurymen being members of parochial boards. Legislation should be on lines that were elastic, and made to apply to different parts, not only to places basking in the sunshine of civilisation like Edinburgh and the other towns he had mentioned, but to places where the days were short and the nights long, as in the North. As to public inquiry, wherever they had a litigated case they were sure to have the facts on both sides brought out, but where there was no contradictor, as in lunacy cases, it became the duty of the judge to protect the alleged lunatic, and he must, so to speak, act as counsel to that party. The judge must be the panel's counsel. That was what he attempted to do himself. It struck at the root of Dr Clouston's contention that the sheriff should only act ministerially. Medical men would also feel the benefit of this judicial treatment of lunacy applications, because the subscription of the sheriff *causa cognita* would prove a protection to medical men who signed the certificate. The want of the magisterial interposition was, he considered, a cause of the troubles in England. There were other points well worthy of notice in Dr Clouston's suggestive and discursive paper into which, had time permitted, he might have gone. No harm, but on the contrary much good, would result from full and free discussion of such subjects between legal men and the medical profession. He had to thank the gentlemen of that profession who were present for permitting him to make these few remarks.

Dr Haddon expected to have heard something in the paper regarding the necessity of making the study of insanity compulsory in the schools. He thought this a most important matter, and one that should be attended to without delay.

Dr Affleck considered that in the matter of certificates the profession should be protected against vexatious proceedings, but the public were also entitled to protection from ignorant and careless practitioners who might unite to certify on wrong grounds. It would never do that such practitioners should be able to take shelter behind the sheriff. When they heard that there had been no prosecution in Scotland, it showed how well the thing had been done on the whole by practitioners in this country. He considered the granting of the certificates separately was quite an unnecessary piece of formalism, because it was often a very difficult matter for one of the certifiers at least, who might never have seen the patient before, to get at facts indicating his insanity, though it might be easy enough for the family attendant to do so. It was only right that the medical

men should have a consultation beforehand. He agreed with Dr Haddon that the profession should be well educated in the evidences of insanity.

Dr Maclaren (Larbert) considered it ludicrous to expect medical men to go into the minute details of certificates, and to run the risks they did without providing for their training in these matters during the regular curriculum. The tyranny of parochial boards was unfortunate. They had the matter entirely in their own hands. He knew of one instance where the board had insisted on their medical officer providing not one, but both certificates, and he had to go about among his medical friends begging them as a personal favour to sign the second one for him. A great deal had been said about confining patients in asylums. He wished to know if the law made any provision for their apprehension after the certificates were signed.

Dr Allan Jamieson said that when he practised in Berwick-on-Tweed, the Border practitioners, even those of them who possessed Scotch degrees or diplomas, found that their certificates were not accepted, because of their living on the opposite side of the Border to that in which the asylum to which the patient was consigned lay. Many of the patients from Berwick were sent to Scotch asylums, and in such circumstances practitioners from the Scotch side had to be called in to certify. The matter had been referred to the Commissioners, but nothing had been done to remove this disability under which they lay.

The President congratulated the meeting on the great concensus of opinion that the teaching of insanity should be made a compulsory part of medical education, and that the teaching should be in connexion with some asylum. In no other way could we hope to be spared the recitals of those horrifying cases of murder and suicide so frequently met with in the public press, and which might have been averted by more rapid diagnosis on the part of the medical attendant. He remembered the unfortunate case of the well-known Hugh Miller, who was permitted to go about without due control, and armed with a revolver, when he was only too manifestly labouring under delusions of a dangerous character. Had ordinary precautions been taken, and Mr Miller been placed under medical control, in all probability a valuable life might have been spared and a public scandal avoided. Fully twenty years ago the extra-mural school of this city took action, and provided instruction on mental diseases at our large asylum; and lately the University following suit, though tardily, has established a special lectureship on the subject under the care of Dr Clouston. The profession, however, labours under other disadvantages of a technical nature, some of which have been alluded to. One which has always appeared to him as involving a certain amount of hardship, is the very limited space afforded the medical man on the certificate paper for the statement of the facts observed by

himself, or ascertained from others, which enable him to certify the case. In courts of law and elsewhere these certificates have been held up to ridicule, owing to the imperfect statement of the grounds of our medical opinion. Is it to be wondered at that in the case of aged practitioners, whose sight is defective and whose writing is in large characters, that with such limited space these certificates are open to criticism? At one of the evening meetings in the College of Physicians, Dr Skae favoured the company by reading some certificates which had come officially into his hands. One bore that the fact upon which the opinion of unsoundness of mind was granted was that the party "wagged his head." It so happened that these words were written in half-text, and occupied all the available space at the command of the medical man, who was advanced in years. The remedy is easy. Let the vacant page at the end of the sheet be utilized, and let each certificate occupy one full page. The controlling lunacy authorities should see to this, and thus relieve the profession from a difficulty which all must feel who have ever certified such cases. Some of the speakers had alluded to the difficulty experienced in dealing with homicidal patients who had as yet only indulged in threats, and reference had been made to the method of certifying such as criminal lunatics. This procedure was peculiar to Scotland, and was one which enabled relatives to protect themselves from imminent danger, and at the same time to secure that the lunatic would not be let loose upon them until a satisfactory cure was effected. The process unfortunately entailed, in the case of paupers, some expense on the parochial boards, and they had sufficient influence with the Legislature to have a supplementary Act passed in 1862, allowing the inspector of poor to take charge of criminal lunatics. The result now is, that the special circumstances of the cases are soon forgotten; and as the expense of maintenance has to be borne by the parish, any asylum superintendent has no difficulty in setting these patients free. Dr Yellowlees stated that in the West an actual assault must first be committed to justify the detention of a person as a criminal lunatic. In Edinburgh this is not necessary. There must, of course, be a charge of some kind to enable the police to interfere; but it is found here that it is sufficient if, say the wife informs the officer that the patient has threatened her, or that her life is in danger, so long as he, the husband, remains in the same house. The other day a case occurred which illustrates the subject well. A man who had been an inmate of several asylums, and had always managed to make his escape, came suddenly home, to the great terror of his wife and family, whose lives he had repeatedly threatened. Being a pauper, none of the incarcerating parishes had any interest in interfering, and they turned a deaf ear to the entreaties of the wife. The lunatic remained indoors, and constantly watched the entrance of the house and also the movements of his wife, who ultimately managed,

through a medical man, to send him (the President) a statement of her case. To the wife it was explained that she must charge her husband in the manner indicated. When the officers of police obtained entrance, the man was found seated at a table taking his tea, with five knives laid out before him. His family were cowering in the various corners of the apartment in abject terror. The apprehension, he is convinced, was not made a moment too soon; and now, most unfortunately, according to the present state of the law, the case is at the disposal of the parochial authorities, who, after a short detention, might obtain the liberation of this cunning and self-possessed lunatic. With Dr Affleck's interpretation of the words in the certificate, "separately from any other medical practitioner," that they forbade consultation between the two certifying practitioners, he could not agree. There was nothing to hinder the doctors, before and after their examination of the lunatic, to meet, compare notes, and come to an understanding on the case.

Dr Clouston said he would not make a lengthened reply because of the lateness of the hour. He had to thank the President and the Society for the hearty way in which his paper had been received. After the opinions expressed as to the position of the sheriff, he felt it would be useless to say more on that point. He ought to explain that when he proposed to allow certain patients in asylums a judicial trial as to whether they were insane or not, he only meant this to apply to a very few exceptional cases. In answer to Dr Maclaren's question he might say that the warrant authorized the transmission as well as the detention of the patient. In regard to the point raised by Dr Jamieson, the opinion of the law officers of the Crown had been given that, as English medical men could not be prosecuted in Scotland, and *vice versa*, under the lunacy acts for false certificates, it would be better to refuse their certificates for Scotch asylums. Still the sheriff was the sole judge in this matter, and it frequently happened that he accepted both English and Irish certificates.

2. ON THE POSITION OF THE EMPTY AND DISTENDED BLADDER IN THE MALE CHILD.

By J. Symington, M.B., F.R.C.S. Ed., Lecturer on Anatomy, School of Medicine, Edinburgh.

The following communication is intended as a contribution to our knowledge of the position of the bladder in the male child. The importance to the surgeon of correct ideas on this subject, more especially in connexion with catheterism and lithotomy, is very obvious. The special difficulties and dangers associated with the latter operation in children, on account of the anatomical peculiarities of the parts, are discussed by all writers on this subject. If, however, we refer to works on anatomy and surgery, the

anatomical instruction we obtain is vague and unsatisfactory. We find it stated that the bladder in the infant is an abdominal rather than a pelvic organ, and that after birth it gradually descends into the pelvis, yet but little information will be gained as to the rate of its descent, and at what age it acquires the position characteristic of adult life. A few quotations will suffice to serve as examples of the way in which the subject is generally dismissed. Sir Henry Thompson[1] writes, "The bladder in children is an abdominal organ rather than a pelvic one." Spence,[2] "In performing the lateral operation on young children it is important to bear in mind that the bladder lies high." Quain,[3] "In infancy the bladder is pyriform when distended and lies chiefly in the abdomen." More details are given by R. Harrison in his article on "The Urinary Bladder" in Todd's *Cyclopædia of Anatomy and Physiology*. He says, "In the fœtus and infant of a year old the bladder in figure more resembles that of a quadruped; when distended, it is pyriform, like a bottle or a flask inverted, the larger end, or the superior fundus, being in the abdomen, and the smaller extremity tapering into the urethra. This is the only portion in the pelvis; at this age its vertical axis greatly exceeds its other diameters, and even when empty the greater portion of it is in the abdomen. As the child increases in years and size, its pelvis expands, the bladder gradually ascends into this region, and in the same proportion its lower fundus enlarges, so that at about six or seven years of age it presents a more oval form, both extremities being nearly equal, and very little of it rising above the pubes, unless when distended. From this period it continues to acquire gradually the adult figure, that is, its inferior fundus and body enlarge, while the superior remains stationary; hence it becomes shorter in its proportions, and broader below, so as to assume the triangular shape when empty, and the ovoid when distended."

Drawings in illustration of this subject are even more defective than the verbal descriptions. Notwithstanding the numerous and valuable works that have been published of late years on topographical and surgical anatomy, there are no accurate drawings of frozen mesial sections of the male pelvis between infancy and adult life. Rüdinger,[4] in a sagittal mesial section of a new-born infant, gives an excellent representation of the position of the bladder, and Henle[5] also figures a section of a male pelvis at the same age. Jarjavay[6] represents a mesial section of the pelvis of an infant three months old.

There are very few mesial sections of the male pelvis in children

[1] *Lancet*, 8th March 1862, p. 246.
[2] *Lectures on Surgery*, 2nd ed., vol. ii. p. 1630.
[3] *Elements of Anatomy*, 9th ed., vol. ii. p. 662.
[4] *Topographisch-chirurgische Anatomie des Menschen*, iv. Abtheilung, Tafel xi.
[5] *Handbuch der Anatomie des Menschen*, fig. 131.
[6] *Recherches Anatomiques sur l'Urètre de l'Homme*, plate iv. fig. 2.

above this age, and those that have been published are of little value. J. Houston[1] has a mesial section of the pelvis of a male child aged two years; but the method of preparation, which consisted in the removal of the pelvis, the distension of bladder and rectum with spirit, and cutting a section after hardening in this fluid, renders it of little use in determining the position of the bladder. Pirogoff[2] gives a sagittal section of a male child aged ten years; but the drawing is very rough, and the bladder, urethra, and anus do not appear to have been opened. He gives a better view of a section of the pelvis of a lad aged 17, but even here the position of the anus is not shown.

There can be no doubt but that sections of the frozen cadaver are the best means at our disposal for the accurate determination of the position and relations of parts one to another, and so far as the bladder is concerned there are no reasons to suppose that it alters its position, in any essential respect, after death.

In the course of an investigation into the topographical anatomy of the child, I have made several sections of the male bladder, which I trust will prove not only interesting to the anatomist, but also useful to the practical surgeon. The specimens I intend specially to describe in this paper are four mesial sections of children, all of whom were almost exactly the same height, thirty-nine inches, and about the same age, five and six years. In all these cases the entire body was frozen. In two of them vertical mesial sections were made of the whole trunk, in the others transverse sections were made at the level of the umbilicus, and the pelves were divided by mesial sections. In two of them the bladder was empty and contracted, in the other two it was distended with water.

Believing that illustrations of such sections are calculated to give a much clearer idea of the position of the bladder than can be obtained from written descriptions and tables of measurements, I have had prepared drawings of two of the specimens, one with the bladder empty, and the other with it distended (see Plates, Figs. 1 and 2). The figures are life size, and made from tracings taken from the sections while frozen. The coils of intestines, which were above the bladder and rectum, have not been represented.

Position of Empty Bladder.—This is shown in Fig. 1, which gives a view of a mesial section of a child aged five years. In this case the whole body was divided in the same plane. The bladder contained only a few drops of frozen urine and was firmly contracted. It will be seen to be situated behind the upper half of the pubic symphysis. A straight line from the upper edge of the symphysis to the tip of the coccyx passed just below the orifice of the urethra, and one from the same point in front, backwards to the juncture

[1] *Views of the Pelvis, etc.* Dublin, 1829.
[2] *Anatome Topographica.* Petropoli, 1859.

of the third and fourth pieces of the sacrum, nearly corresponded to the upper surface of the bladder. The prostatic part of the urethra was 13 mm. in length, and the membranes 5 mm., and the vesical orifice of the urethra was 20 mm. above the level of the lowest part of the bulbous portion of the urethra. The cavity of the bladder was triangular on section, one angle was directed forwards, another backwards, and the third, corresponding to the opening into the urethra, downwards. The rectum was empty. The other specimen with the bladder empty and contracted was from a child of the same age and height, but rather stouter. The bladder had practically a similar position and shape to that in Fig. 1, but it was quite empty, and the upper surface of the bladder was more convex.

In Fig. 1 the peritoneum lined the anterior abdominal wall down to the upper border of the pubic symphysis, and at this point it turned backwards, covering the upper surface of the bladder. At the juncture of the superior and posterior surfaces it formed a free fold, and then descended between the bladder and rectum to within 4 cm. of the anus. The peritoneum in the other specimen extended downwards in front slightly behind the symphysis; but it did not reach so low behind the bladder, and the distance from the very shallow recto-vesical pouch to the anus was 5·2 cm. In this case there was a fold of the peritoneum like that shown in Fig. 1, but a little longer. I have always found one or more folds of this kind in cases where the bladder was empty and contracted; but I have not seen any references to them. They will gradually be obliterated as the bladder becomes distended with urine.

It is difficult to make a satisfactory comparison between the position of the bladder in these two cases, and that which it occupies *under similar conditions* in the adult, on account of its usual position in the latter not having been determined. Almost all the mesial sections of the adult male pelvis that have been published are from cases in which the bladder was more or less distended. Jarjavay,[1] plate vi., gives a section with the bladder empty and contracted, in which it is lower in the pelvis than in my specimens; but it looks very much as though it had been allowed to fall downwards and backwards before it was drawn.

Position of Distended Bladder.—The positions and relations of a distended bladder are seen in Fig. 2, which represents a section of a child about six years old. The whole trunk was divided by a mesial section. Before freezing about two ounces of water was slowly injected into the bladder. A comparison of the two specimens demonstrates clearly that the bladder has not only risen into the abdomen, but has also pushed its way down towards the perinæum, and that this has been accompanied by a marked shortening of the length of the prostatic portion of the urethra.

[1] *Op. cit.*

This was 7 mm. in length as compared with 13 mm. in the other case. The vesical orifice of the urethra was 10 mm. above the lowest part of the bulbous portion of the urethra, while in the specimen with the bladder empty it was 20 mm. Again the distance of the urethral orifice from the anus in Fig. 1 was 4 cm., in Fig. 2 about 3 cm. The peritoneum was reflected from the abdominal wall on to the bladder 8 mm. above the pubic symphysis, and the very deep recto-vesical pouch reached to within 18 mm. of the anus. In this child the lower half of this pouch was occupied by serous fluid.

In another male child, about five years old, I injected three ounces of water into the bladder before freezing. In this case the descent of the bladder towards the perinæum, and the shortening of the prostatic part of the urethra, although distinct, was not so marked as in the former case. The length of the prostatic part of the urethra was 10 mm., and the internal orifice of the urethra was 17 mm. above the level of the lowest part of the bulbous portion of the urethra. There was a marked difference in the position of the part of the bladder lying behind the urethral orifice in the two cases with the bladder distended. In the one represented in Fig. 2, this part of the bladder wall passed nearly horizontally backwards for about 2 cm. before it began to turn upwards. In the other case the lower end of the bladder was not so large, and the part behind the urethral orifice passed directly upwards and backwards. In this case the bladder reached as high as the base of the sacrum. The peritoneum was reflected from the anterior abdominal wall on to the bladder 2·7 cm. above the symphysis pubis. The lower end of the recto-vesical pouch of peritoneum was much higher than in the other case, being 4·3 cm. above the anus.

With regard to the differences in position of the two distended bladders, I think it may be partly attributed to variations in the shape and position of the coccyx. In the specimen shown in Fig. 2 the coccyx is directed downwards and slightly backwards, while in the other specimen it curved somewhat forwards. In the latter case it would give more support to the posterior part of the pelvic floor. As the bladder is gradually distended it will naturally tend to extend in the direction of least resistance, and the descent of the fundus of the bladder in the one case may have been due to a weakness of the pelvic floor.

Garson[1] investigated, in the Anatomical Institute at Leipzig, the "Displacement of the Bladder and Peritoneum in the Male by Distension of the Rectum." He showed that by distension of the rectum the bladder was pushed up towards the abdomen, the prostatic and membranous portions of the urethra being stretched, the former to about double its normal length. Since then several surgeons (Petersen, Ferrier, etc.) have advocated and employed distension of the rectum in order to raise the bladder before

[1] *Edin. Med. Journal*, Oct. 1878.

PLATE. 1

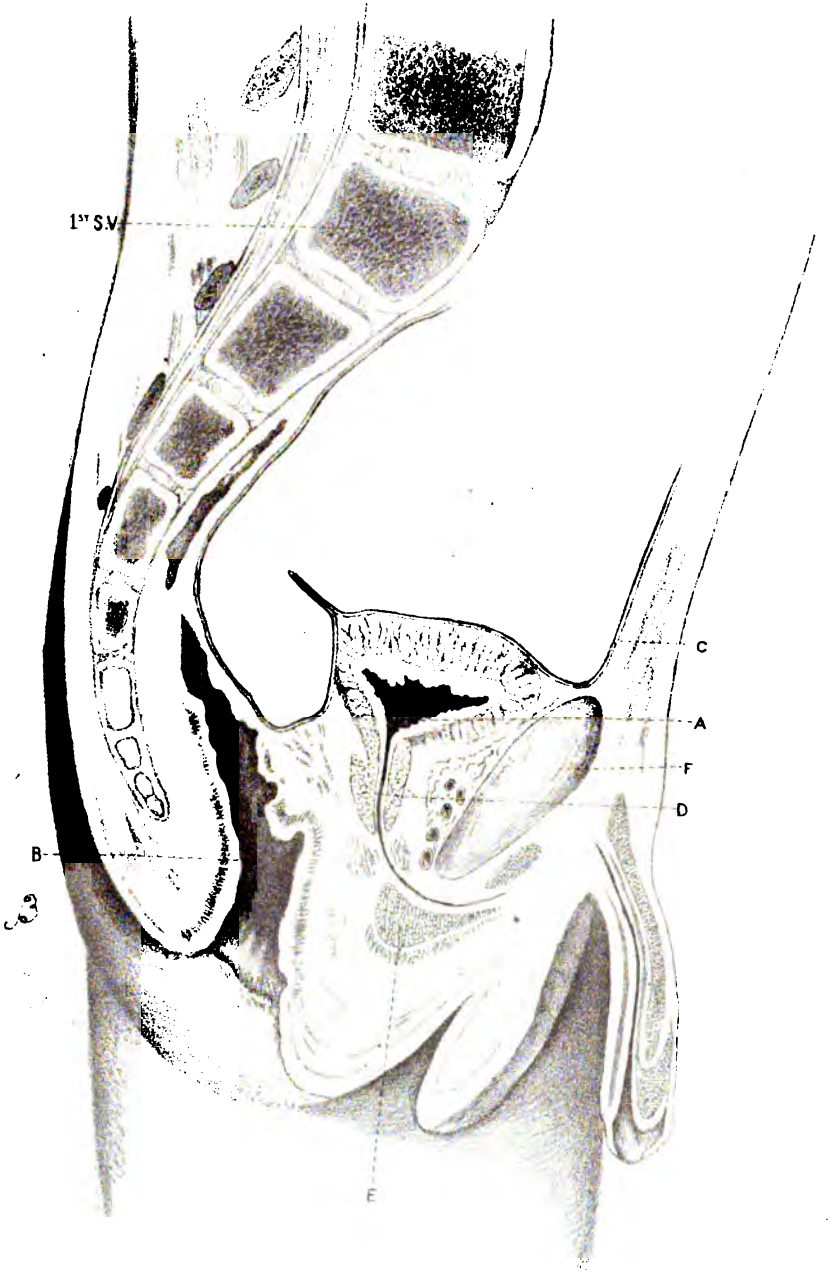
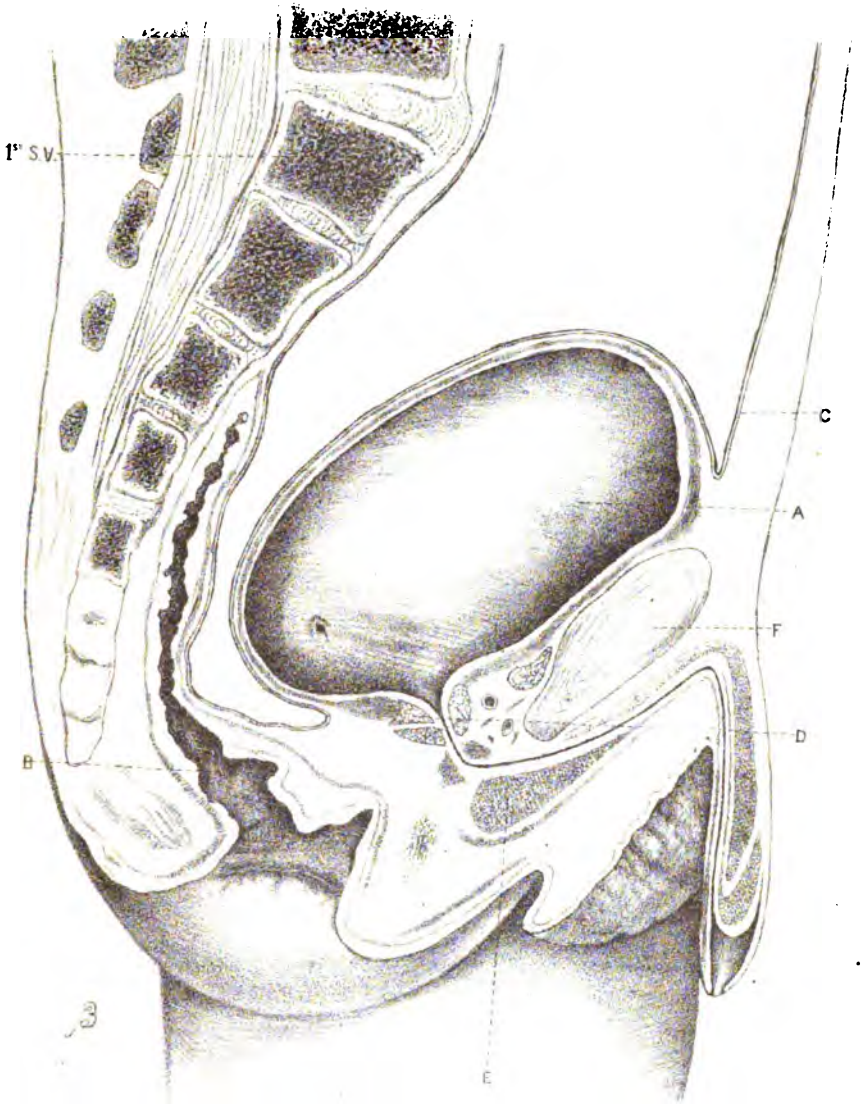


PLATE.2



performing supra-pubic lithotomy. We usually think of the bladder during distension as simply rising into the abdomen, and but little attention appears to have been paid to the possibility of its passing also towards the perinæum and diminishing the length of the prostatic part of the urethra. The only reference to this subject that I have noticed in anatomical works is in Rüdinger's *Supplement zur Topographisch-chirurgischen Anatomie des Menschen*, 1879. In plate iii. he figures a mesial section of a male body in which the bladder was greatly distended. In the explanation of this plate he directs attention to the low position of the base of the bladder. The plate also shows very distinctly a marked shortening of the prostatic and membranous portions of the urethra. This subject is obviously worthy of more attention than it has yet received.

EXPLANATION OF PLATES.

FIGURE 1. Mesial section of pelvis of boy aged 5 years. Bladder nearly empty and contracted.

FIGURE 2. Mesial section of pelvis of boy about 6 years old. Bladder distended with two ounces of water.

A. Bladder.	D. Prostate.
B. Rectum.	E. Bulb.
C. Peritoneum.	F. Pubic symphysis.

Meeting VI.—April 1, 1885.

DR H. D. LITTLEJOHN, *President, in the Chair.*

I. ELECTION OF HONORARY MEMBER.

Charles West, Esq., M.D., F.R.C.P., London, was elected an Honorary Member of the Society on the nomination of the President and Council.

II. EXHIBITION OF PATIENTS.

1. *Mr Joseph Bell* showed—(*a.*) A female patient on whom he had operated for a FATTY AND FIBRO-CELLULAR TUMOUR[1] on 3rd Dec. 1884. The tumour itself had been exhibited to the Society on the evening of the day on which it was removed. It had involved the right parotid gland, and projected into the pharynx. She had made an excellent recovery, both the tracheotomy wound and the incision for the removal of the tumour having healed well and rapidly. The tracheotomy had been rendered difficult because of the presence of a cystic goitre. The scar was, however, a very slight one, and the pharynx now presented no appearance of

[1] See page 22.

having been occupied by the tumour. (*b.*) A young man whose right OS CALCIS he had removed sub-periosteally some three years before. The whole of the bone along with a small portion of the astragalus had been taken away, but the reproduction of it had been thorough, and the patient himself had volunteered the statement that his right foot was now the stronger of the two.

2. *Dr Allan Jamieson* showed a young woman suffering from BELL'S PARALYSIS, which he believed to be due to a cause frequently mentioned in books, but not commonly met with, viz., syphilis, and most probably syphilis inherited. He based his belief on the grounds,—first, that her mother had one previous child who died in infancy; second, that the girl herself had had ulcers about the angles of the mouth, the scars of which could be plainly seen; and, thirdly, that she had suffered from double keratitis, for which a double iridectomy had been performed three years ago by Dr Reid of Glasgow. It was an ascertained fact that keratitis did not occur in association with acquired but with hereditary disease. She was first seen on the 20th of March last, the paralysis having existed for nearly one month. She was at once put on mercurial treatment, and the paralysis was disappearing. On turning up some of the authorities he found they only mentioned syphilis without discriminating between the hereditary and acquired forms. Curiously he had another case of this nature due to acquired syphilis at the time this patient came under notice.

3. *Mr A. G. Miller* showed—(*a.*) A case of MULTIPLE INJURIES, including fracture of the neck of the scapula, caused by a railway accident. The patient, a young man, was shunting some waggons on 22nd Nov. 1884, when he was knocked down against the rails. On admission at the Infirmary he was found to have—(1), a triangularly shaped wound over the right eye, raising a flap which was but slightly attached by its outer angle; (2), a cut behind the angle of the mouth on the left side; (3), a cut behind the left mastoid process about 3½ inches in length; (4), the nose dislocated; (5), dislocation and bruise of the right shoulder; (6), fracture of the femur at the junction of the middle and lower thirds. The extravasation of blood accompanying the fracture of the femur was so great that extension without splints was applied. The bone united, however, in first-rate position, and the patient was already able to walk with but a slight limp. The dislocation of the shoulder was reduced by the house-surgeon, but there being still deformity Mr Chiene was asked to see the case in the absence of Mr Miller. He considered that the dislocation had probably been complicated by fracture of the neck of the scapula. The patient suffered so much pain when any attempt was made to bandage his arm that he was allowed to lie in bed with it unbandaged. On 30th Jan. an examination of the shoulder was

made. There was flattening in the deltoid region. The acromion process appeared as the most prominent point in the shoulder. There was flattening in the subspinous region, as also a hollow between the head of the humerus and the acromion process. The head of the bone lay slightly forwards from its normal position. At times distinct crepitus could be felt. Later it was noted that when the arm was supported at the elbow there was an interval of an inch between the coracoid process and the clavicle, but when unsupported the difference between those points was 1¼ inches. From acromion to olecranon the right arm measured 15¼ inches, the left 14¾ inches. The head of the bone still lay downwards and forwards. It was nearly in the position of a sub-coracoid dislocation, but in proof that there was no such dislocation he could touch his opposite shoulder quite easily, put his arm behind his back, and touch the back of his head. The crepitus which was present pointed to fracture. The question was what part of the scapula had given way—the glenoid alone, or the glenoid plus the coracoid process? If the latter, the coracoid ligament must also have given way. The movement of the coracoid showed that the ligaments must have given way or been much stretched. The coracoid moved with the head of the humerus. He considered that the fracture included the coracoid process, and that fibrous, not osseous, union had taken place. (*b.*) A case of EXCISION OF BOTH TESTICLES. Sixteen months previously the patient had been struck by a stone on the lower part of the abdomen and scrotum. He was admitted to Mr Miller's Wards in October 1884, when, in addition to scrofulous disease of the testicles, he was found to be suffering from phthisis. Dr Wyllie examined him and found dulness of both apices extending to the third rib on the right side and the second on the left. The breath sounds above the clavicle were harsh and vesicular on both sides. On the left several moist sounds were heard. At the end of October patient went to Berwick-on-Tweed. He was re-admitted on 31st December. On 23rd January both testicles were removed by means of V-shaped incisions, which included a considerable portion of the skin of the scrotum. At the same time a radical cure of a right hernia was made. He had previously worn a truss, but it had caused him so much pain in his spermatic cord that he was obliged to do without it. The wounds healed with the formation of scarcely a drop of pus. Since the operation the condition of the chest had greatly improved, the moist sounds and dulness having disappeared. His voice had also gone up several notes.

III. EXHIBITION OF DRAWINGS AND PATHOLOGICAL SPECIMENS.

1. *Mr A. G. Miller* showed two drawings from a work by Dr Brossard of Lyons demonstrating THE POSITION OF THE HAND IN FRACTURE OF THE LOWER END OF THE RADIUS. Dr Brossard had

made some experiments on the position of the hand in certain cases of fracture of the ulna. These two sections struck Mr Miller as demonstrating very forcibly the mechanism of fracture of the lower end of the radius. The first showed the bones of the carpus in the position in which fracture usually took place, the arm pronated, the hand dorsi-flexed and turned towards the radial side, which is the position a person places his hand in when falling forwards or backwards. The close approximation of the scaphoid and semi-lunar bones to the radius showed how the force of a fall on the hand is directly applied to the lower end of the radius.

2. *Mr Joseph Bell* showed—(*a.*) An EXOSTOSIS OF THE LOWER END OF THE FEMUR, which grew upwards and backwards in the line of the adductor tendon. It had a long neck, only a small part of which was free from cartilage. (*b.*) BILIARY CALCULI removed from the gall bladder of a heavy, fat woman, weighing about 17 stones. She had a sinus in the middle line two or three inches above the umbilicus, with a small discharge of sanious pus. The history of the case shortly was that an umbilical hernia had formed eleven years before. Fifteen months ago she had an attack of "inflammation of the bowels." Four weeks later, while she was still confined to bed, a small swelling formed in the position of the sinus. This enlarged in about a week to the size of an orange. It went down under poultices, but never entirely disappeared. In June of last year, after severe exertion, the swelling became painful and increased in size. In August it burst of itself, and what she described as ordinary matter came out of it. Since then some small round whitish bodies have been discharged from it. These crumbled like dry mortar between the fingers. After admission to the Infirmary a probe passed along the sinus showed the presence of a hard rough body. This was cut down upon, and several fragments of gall stones and two unbroken ones were removed.

3. *The President* showed—(*a.*) THE TONGUE AND AIR-PASSAGES of a young man who had been suffocated during a fire. On opening the body it was clear that he had suffered from carbonic acid and carbonic oxide poisoning. The air-passages were dotted all over with the black matter he had inhaled, and there was also the intense injection to which attention had first been drawn by Caspar. The blood showed the signs of carbonic oxide poisoning. It had been asserted that he was under the influence of alcohol, but the examination of the stomach failed to bear out this statement. (*b.*) The parts from a case of SUICIDAL WOUND OF THE NECK. The suicide was a man over 60 years of age, and the weapon was an ordinary table-knife. He had first of all wounded the crico-thyroid membrane, and then cut into the thyroid cartilage. Had he not been seen to inflict the injuries himself, their position might have led to some suspicion, self-inflicted injuries being usually much higher up.

IV. Original Communication.

THE CESSATION OF RESPIRATION UNDER CHLOROFORM, AND ITS RESTORATION BY A NEW METHOD.

By R. Milne Murray, M.A., M.B., M.R.C.P. Ed., Physician for Diseases of Women, Western Dispensary, and Lecturer on Practical Gynecology, Edinburgh.

Notwithstanding the manifest tendency during recent years to employ, for purposes of anæsthesia, sulphuric ether instead of chloroform, it is obvious that the latter drug is with many surgeons still the favourite. So far as I can learn, Scottish surgeons, with few exceptions, employ chloroform, only turning to ether in special and comparatively rare cases. On the other hand, in England and America the case is becoming rapidly reversed, the majority of operations being performed under ether, and the exceptional ones under chloroform. This recent return to the older anæsthetic is to be explained by the growing impression that in point of safety ether has the advantage, while much of the difficulty of its administration is being overcome by improved apparatus and better methods of exhibition. Still, in spite of an admitted degree of greater safety, the administration of ether is undoubtedly attended by more than one disadvantage, and there is a suspicion, probably well founded, that all the risks attending its use do not end with its administration, but that to the list of inconvenient results must be added not a few pulmonary and other complications which may follow, and which may reasonably be attributed to its action.

Be this as it may, chloroform is possessed of many advantages, not the least of which is its easy administration and its quick action; and it will no doubt continue to be employed by many until at least some substance more satisfactory than any yet brought forward is met with. Certainly I should think that there are but few obstetricians who would be enticed from the use of chloroform by any of the advantages which may be claimed for ether in general surgery. The extreme inflammability of the vapour, and the bulky apparatus required for its satisfactory administration, are in themselves sufficient to prevent its general use in this department. Moreover, all agree that for children chloroform rivals all other anæsthetics in the efficiency and safety of its action. Such being the case, it is probable that these advantages will secure for chloroform a very considerable employment in surgery and obstetrics, and will render its action a study of deep interest to the practical surgeon. On this ground I have ventured to bring before the Society an account of some observations I have been able to make, and which seem to me to involve points of some practical interest in relation to the physiological action of chloro-

form. I have no purpose of pleading the cause of one anæsthetic over the other. My object is merely to call attention to some features of the action of chloroform which seem to me to have been overlooked in previous investigations.

When we turn to the records of observations on the action of chloroform, we are struck with the amount of attention bestowed on the effect of its administration on the action of the heart and blood pressure, and with the careful records which have been made of this; while, at the same time, the effect on the respiration is indicated in comparatively loose terms, and graphic records are seldom given. It would seem, indeed, that while the potency of chloroform as a respiratory poison was recognised from the first, the difference of its action as compared with ether on the heart and circulation has engrossed the attention of observers. Thus it has come about that less attention has been paid to the respiratory changes brought about by its influence than to the remarkable effects produced on the circulatory system.

The records of these latter changes are ample and accurate. Thus the Glasgow Committee[1] concerned themselves almost wholly with the circulation, and give several important manometric tracings of blood pressure, but none of the respiration, during any of the experiments, and their account of the respiratory phenomena is by no means so full as it might be. And they are by no means singular in this respect, the same meagreness of detail being characteristic of other reports.

As my observations have shown me that the process of respiratory cessation in animals does not always present the same type, but is subject to variations of marked character, I have thought I might be justified, in view of this deficiency of existing records, in bringing these observations under notice.

My experiments were conducted on rabbits; and while I do not mean that we can justifiably reason directly from them to other animals, yet it seems unlikely that the striking features of the action of chloroform on their respiratory process will not be repeated in one way or other in higher animals.

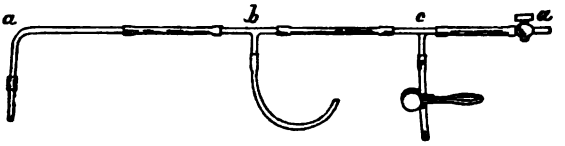
The series of experiments under discussion have been undertaken as a side issue in a research into the action of the uterus in rabbits, on which I have been engaged for some time. In my experiments on this organ I found that very striking changes occurred in the respiratory rhythm as a result of stimuli applied to certain parts of the pelvic viscera and lumbar cords of rabbits; and it was in the course of the endeavour to determine the exact nature of these changes that my attention was arrested by the behaviour of the respiratory mechanism under the influence of chloroform.

As I could find in the literature of the subject no accurate description of the respiratory phenomena in anæsthesia, I had to

[1] *British Medical Journal*, vol. ii., 1880.

proceed to its investigation. I shall have occasion to refer later on to the opinions scattered through the text-books and lectures on the subject, but meantime proceed to describe the mode of observation adopted.

Mode of Experiment.—The animal was fully anæsthetised by chloroform by means of a cone containing lint soaked in the fluid, and held over its nose. It was then secured to the holder, and tracheotomy performed, and a bent glass cannula tied in. The time required for this part of the operation varied with the greater or less susceptibility of the animal to the vapour. The variations were considerable, and from the time of first administration of the vapour to the completion of tracheotomy a period of from 5 to 15 minutes elapsed. The cannula *a* was connected by short pieces of



rubber tubing to two glass T tubes *b* and *c*. The branch of *b* was connected with a tambour or other recorder, and that of *c* was employed for administering the chloroform. This latter tube could be closed by a pinch-cock. The main tube ended in a vulcanite tap *d*.

By means of this vulcanite tap the effect of the air current on the tambour could be regulated: thus more or less extensive movements of the tambour lever could be obtained according as the tap was partially closed or opened. In the earlier observations a tambour writing on a continuous roll of paper by means of an ink-pen was employed. In the more recent ones, a slow moving smoked surface has been substituted; and instead of the tambour, I have employed a form of water-manometer devised by myself for such purposes.[1] This instrument possesses several advantages over Marey's tambour, which, I believe, render its indications more valuable and accurate. Its chief advantage is, that while it is as sensitive, if not more so, than the tambour for small pressures, it is capable of recording accurately much greater variations than Marey's instrument can indicate. In the latter instrument the limit of elasticity of the rubber membrane is soon reached, when increments of pressure fail to effect proportionate movements of the lever. Thus the characteristic features of many tracings demanding such record are lost. The special nature of the tracings obtained by this instrument will appear further on.

I have explained that the chloroform was administered by

[1] This instrument will be described in an early number of the *Journal of Physiology.*

means of the second T tube. The concentration of the vapour was regulated by the size of the receptacle for the chloroform and the diameter of its neck. The same size of tube was always employed; and if this were dipped into a narrow-necked bottle containing a large quantity of chloroform, the vapour would be in its most concentrated form, whereas were it placed in a large jar containing a small quantity of the anæsthetic on cotton wool, the vapour would be in a dilute condition. Further, the quantity given could be still further diluted by admitting a larger quantity of air by the tap, and by cutting off some of the vapour by partly closing the pinch-cock on the chloroform tube.

I made some experiments by having a Wolff's bottle attached to the main tube, and by connecting the registering apparatus to the other neck of the bottle; but as the results of several experiments did not vary from those of the simpler apparatus, I abandoned this arrangement as being needlessly cumbrous. From such an arrangement it will be seen that every inspiration will draw air out of the tambour, and will be indicated by a fall in the lever, while expiration will force air into the tambour, and will cause a rise of the lever. In the case of the records obtained by the manometer, the opposite is the case. The fall of the lever represents an expiration and the rise an inspiration. The apparatus having then been arranged as described, the animal was allowed to breathe air alone until signs of returning reflex appeared. The respiratory rate now generally fell to 7 or 10 respirations in 10 seconds—*i.e.*, from 40 to 60 per minute, varying chiefly according to the size of the particular animal. The clock was now started, and a tracing of the respiration taken for 10 or 15 seconds. The chloroform tube was then dipped into the receptacle, and the moment noted on the paper. In many of the tracings, the respiratory movement was recorded continuously until cessation. In others (these extending over several hours) the respiration was taken before and after each change in administration or withdrawal, and at regular intervals of time.

In this way I could note the effects of concentrated and dilute vapour, given continuously or intermittently, and I have therefore observed the mode of respiratory cessation under the following conditions:—

1. The continuous administration of the concentrated vapour.
2. The alternate administration of concentrated vapour and air.
3. The continuous administration of the dilute vapour.
4. The alternate administration of the dilute vapour and air.

Further, as I have succeeded in resuscitating the same animal repeatedly after respiratory stoppage, I have noted the effect of the administration of chloroform.

5. After repeated cessation and re-establishment of respiratory movement.

6. After the withdrawal of blood from the vessels.

It is to be clearly understood that the observations I am about to describe deal with the respiratory phases under the influence of chloroform from the supervention of anæsthesia onwards, and have no bearing upon the changes during the period of the early action of chloroform up to the production of anæsthesia. The terms of my license do not permit me to make observations of this nature until the anæsthesia has been induced. The absence of an account of the early stages of chloroform action does not, however, I venture to think, rob the observations about to be described of practical interest, inasmuch as it is undoubtedly *after* the induction of anæsthesia that the grave danger from chloroform arises from oversight or misadventure.

I have already stated that the records of changes in the respiration by previous observers have been more or less indefinite and unsatisfactory, and that we can form from these statements no very distinct idea of the effects produced by the anæsthetic.

In the very elaborate report of the Chloroform Committee of the Medico-Chirurgical Society of London[1] I find the following under "The Effect of Chloroform on the Respiration:"—"The respiratory efforts were at first deep, but by degrees they became more and more shallow. With this loss of depth the respirations for a while retained their unnatural frequency, but after a time became less frequent than natural."

Sansom[2] says, "The breathing is quieted, but the rate of the respiration is scarcely altered, unless the narcotism is pushed to coma."

It would be difficult to reconcile these two statements.

Dr P. Knoll,[3] in an elaborate paper on "Chloroform and Ether," says, "The first change is a long expiratory tetanus, which may last 6 to 10, or even 16 seconds, and may be repeated several times. Respiration then remains a short time greatly retarded, often partly, sometimes entirely, owing to the prolongation of the respiratory phase, then follows the final acceleration, with still deep inspiration, but becoming gradually shallower, until it entirely ceases."

Wood[4] says, "The respirations may be at first rendered slower by chloroform, but after a time are generally quickened, and as inhalation is persisted in, they become more and more shallow, irregular, and distant, and finally cease."

Several other authorities may be cited, but these will serve to show that the general impression is, that after certain phases admitted by some and denied by others, the respiration becomes

[1] *Trans. Medico-Chirurgical Society*, vol. xlvii. p. 323.
[2] *On Chloroform*, p. 63.
[3] *Sitzungb. Wiener Akad.*, 1876.
[4] *Treatise on Therapeutics*, p. 288.

slower, shallower, and finally ceases. As to how far the experiments about to be described correspond with any of those statements will appear later on. It would serve no good purpose to occupy the time of the Society by a detailed description of the individual experiments. I purpose rather to give a general account of the results arrived at, and will only refer to the details of such experiments as may be necessary to establish my conclusions. I shall therefore consider—

I. *The Effect of the Continuous Inhalation of Concentrated Vapour, from the Induction of Anæsthesia until Cessation of Respiration.*

A well-grown rabbit is fully anæsthetised, secured and connected with the recording apparatus as described. Air only is breathed, until the conjunctiva reflex appears, and the respiration has fallen to 6 to 10 in 10 seconds. If the chloroform tube is now dipped into the narrow-necked bottle, containing as much chloroform as will bring it to within half an inch of the tube, the tambour will indicate the following changes:—Soon after the tube is dipped in, a noticeable change occurs in the respiratory rate. The first 2, 5, or 10 respirations may show no signs of change, but at the end of this interval the rate begins to rise. Coincident with this increase in rate we almost invariably find a marked increase of volume. This increase does not take place suddenly, but each succeeding respiration is slightly deeper than its predecessor, and soon the maximum depth is attained. This maximum condition is maintained for a very short period, sometimes for not more than 2 or 3 seconds, and then the volume begins gradually and regularly to diminish. Moreover, at the moment the respiration has attained its maximum volume it has also assumed a definite rate, which is maintained up to the very close of the respiratory movements with almost absolute uniformity.

In a typical case of respiratory cessation, due to the continued action of concentrated vapour, we have the following stages (the animal being fully anæsthetised):—

1. A latent period of 3–5 respirations.
2. A period of quickening rate, and increasing volume.
3. A period of constant rate, and diminishing volume.

The earlier observations being made on continuous paper, the records themselves are too long to be conveniently handed round, and the waves are too far apart (the paper moving rather rapidly) to show the gradations of the phases satisfactorily. But the special features in the change of volume will be shown in the tracings which I hand round, and the variation in rate from administration to cessation will appear in the following table:—

Plate I.

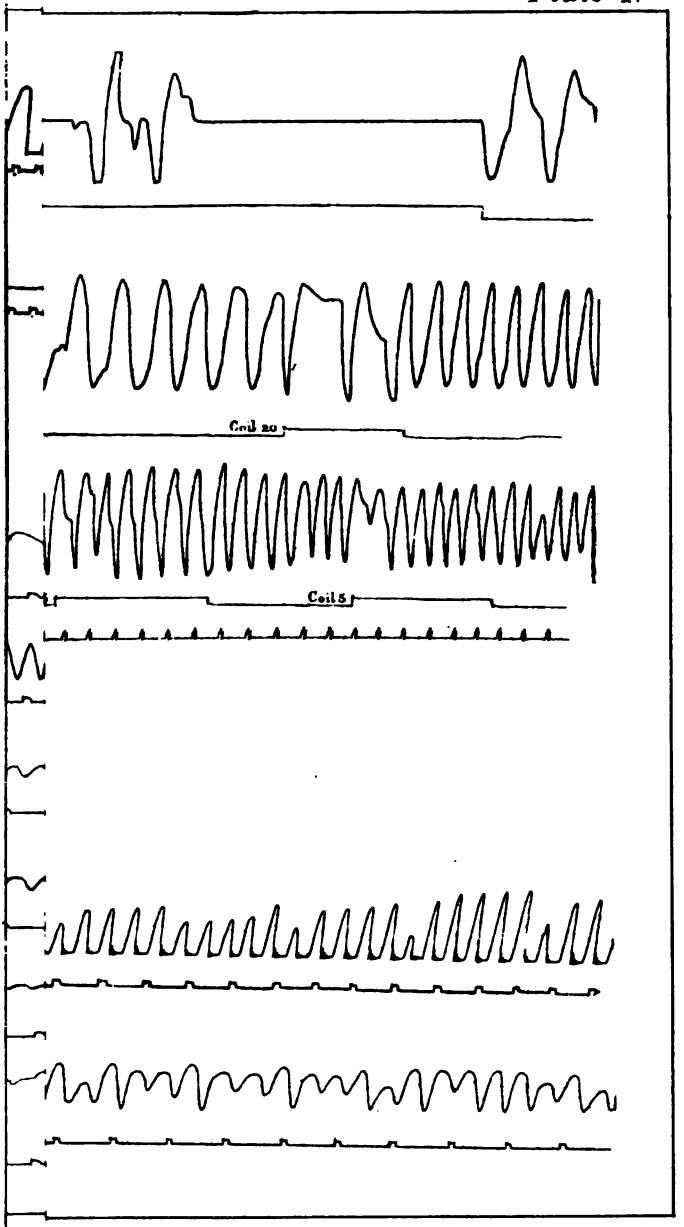


Plate II

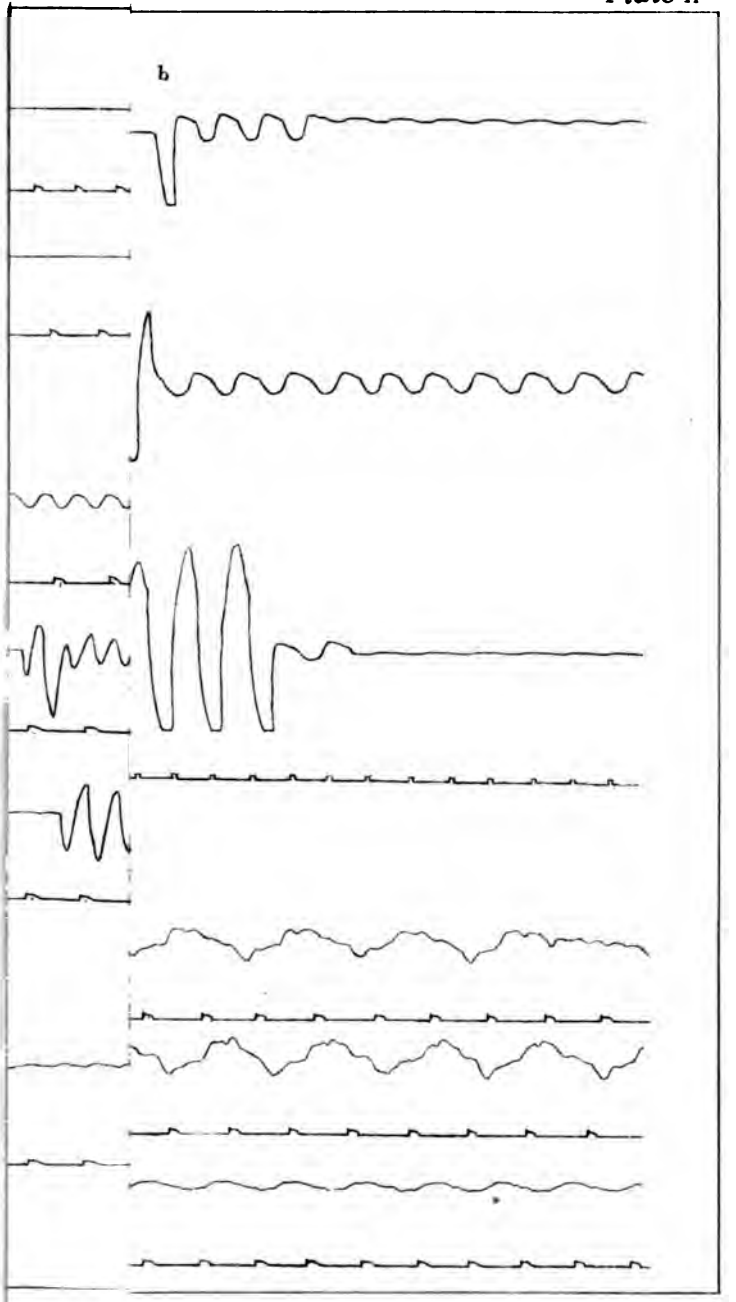


Plate III

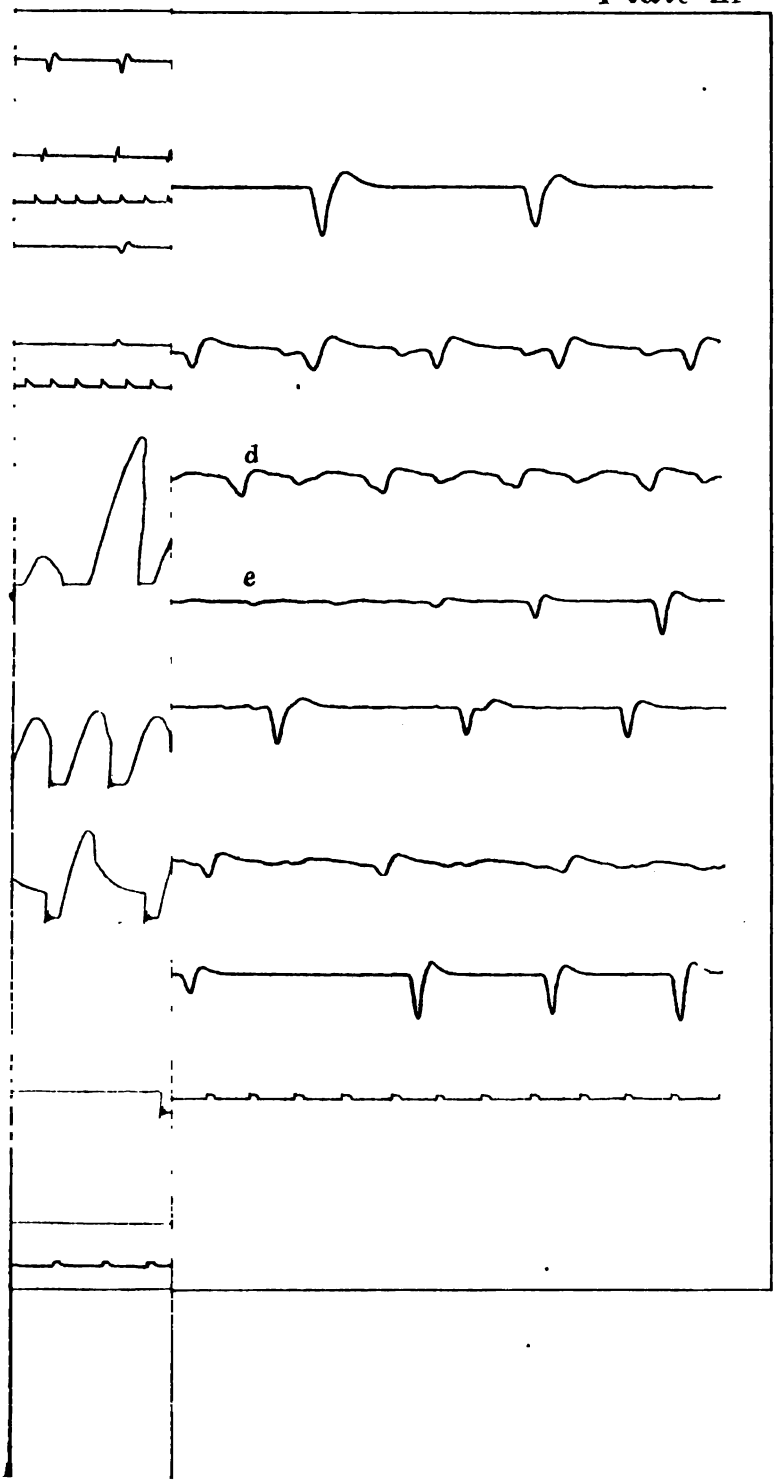


Plate IV

'ABLE showing the Respiratory Rate in Successive Periods of 10 Seconds from Administration of Final Dose till Cessation (Concentrated Vapour).

		I.	II.	III.	IV.
Sex of animal,		F., ¾ grown.	F., full grown.	M., full grown.	F., full grown.
Time from commencement of chloroform,		15 min.	25 min.	18 min.	30 min.
Respirations in 10 seconds before lethal dose,		10	12	10	7
Respirations in successive periods of 10 seconds during administration of lethal dose,	0–10	12	18	13	10½
	10–20	17	18	14	11
	20–30	18	20	14	11
	30–40	23	20	15	10
	40–50	26	20	14	12½
	50–60	26	20	15	12
	60–70	26	20	14	12
	70–80	25	20	14	10
	80–90	25	19	14	12
	90–100	...	20	14	12
	100–110	...	...	14	12
	110–120	...	...	14	
	120–130	...	...	14	

It seems to me unnecessary to detail more examples of this ıature in this fashion. Many more could be given, but they all ›ring out the same feature, namely, that after the maximum rate s established (what I may call the cessation rate), this rate coninues, with scarcely any alteration, until the very end.

It is not possible to give, except by a tedious and elaborate system ›f measurement, a description of the changes in *volume* coincident vith these changes in rate. But a glance at the tracings taken rom the original records which I show, will bring out what I ıean to indicate. Take, for instance, the tracing Figs. 1 and , Plate I., which is the first I obtained, and which was the first › call my attention to the curious uniformity of the rate. The nimal, a full grown doe, had been breathing ether for some hours, nd in order to kill it I substituted chloroform, with the result ıown in the tracing, namely, a gradually diminishing volume, 'ith an almost absolutely constant rate.

Again, the tracing Figs. 3, 4, and 5, Plate I., obtained from full grown buck, shows the same features as I have described. 'he first line, Fig. 3, shows the rate previous to the administration of the fatal dose of chloroform, the rate being 5½ in 10 econds. The second line, Fig. 4, shows the rate and volume 0 seconds after chloroform was begun. At the beginning of he line the volume is deeper than in the first line, and the ate 19 per 10 seconds. Towards the end the volume is diminishng, and the rate in the last 10 seconds of that line is 23. Line , Fig. 5, 30 seconds after Fig. 4, shows the cessation of the

respiration, the volume gradually diminishing, but the rate remaining constant at 22 or 23 per 10 seconds.

But the special features of this form of respiratory death become much more distinct when we turn to the tracings obtained by means of the slow moving drum. In these the whole of the changes are brought together under the eye at one moment.

I first show two tracings, Figs. 40 and 41, Plate IV., taken from the same animal, it having been resuscitated after the first one. The rabbit was a full grown male, which had been 18 minutes under chloroform. After tracheotomy, the respirations had sunk to 12 per 10 seconds, Fig. 40. Chloroform was given at the point marked (*Ch*), and it will be noted that the respiration, which has been somewhat irregular previously, becomes steady, and of from 12 to 14 crests. The volume then sinks slightly, and immediately after begins to increase, the rate being somewhat accelerated. Soon it reaches its greatest volume, then gradually diminishes, the rate being maintained up to the very end. Just before ceasing, the slightest possible increase in volume appears, but lasts a very few seconds, and the end of the tracing shows a gradual diminution and final disappearance of the curves.

A similar description applies to the tracing Fig. 41 which follows. The animal on the conclusion of Fig. 40 was quickly resuscitated, and when the breathing reached 10 to 12 in 10 seconds, chloroform was again given, with a similar result. In this case the deepening at the end is even more marked than in the previous one, and the other features are almost identical.

The tracing Fig. 42 was obtained from a three-parts grown female, and does not require more detailed description. The tracing is much shorter than the previous one, but the feature of diminishing volume and constant rate are clearly brought out.

The tracings Figs. 43 and 44 are similar in main features to those already described, but show some characters which I shall have to refer to shortly. Fig. 43 was obtained from a large male rabbit which had been 40 minutes under chloroform before the tracing was commenced. Air only was breathed up to part marked "air." Chloroform being given, the respiration is unaltered for two or three breaths, then it gradually rises in rate and volume, then the volume diminishes, the rate remaining the same. Unfortunately the drum ran out before the cessation occurred, and the last part of the tracing is not given, but one can safely fill up the gap from the experience of other cases. The special features of this and the tracing Fig. 44 I must leave for subsequent notice.

The period required to produce death by chloroform given in this manner varies with many circumstances, notably with the size of the animal; but it seems a most interesting point that, after the respiration rate is fairly established coincident with the maximum volume being attained, we should have an absolute mainten- that rate down to the very last gasp.

So far as I am aware, this feature of chloroform death is not recorded in the description of any of the numerous experiments which have been made on this anæsthetic. The gradually increasing feebleness of the respiratory movements has been noted by all observers. The diminution of the heaving of the chest-wall is obvious to the most casual onlooker, and it was perhaps natural to assume, in the absence of definite demonstration to the contrary, that the reduced volume was accompanied by a diminishing rate. Such, however, is not the case, so far as a very considerable number of observations seem to show under the condition I have discussed, namely, continuous administration of concentrated vapour.

So far, then, we are justified in concluding that in rabbits a fatal dose of chloroform vapour given under anæsthesia affects the respiratory centre in two stages.

1. A short stage, in which the explosions of the respiratory centre are quickened in rate and increased in intensity—a stage of "acceleration and augmentation."
2. A longer stage, in which the explosions occur at the previous quick rate, but in which their intensity becomes gradually less and less, until they disappear.

While considering the action of such concentrated doses, there are one or two points which demand attention.

1*st*, When we compare more minutely the character of the breathing in the two stages just referred to, we shall find certain characteristic features which are worthy of notice. I have said that there is a *gradual* increase of the extent of movements in the earlier stage, and a gradual diminution in the second. So far as the majority of the earlier observations show, this diminution in the second stage would seem to be perfectly regular, *i.e.*, each inspiration is somewhat less deep than its immediate predecessor. These earlier observations were made by means of tambours furnished with writing points, and were taken on continuous paper at a comparatively rapid rate, and under a very low air pressure. In some cases, when the air pressure was considerable, the tambour indicated a remarkable irregularity in the second stage, in which, while the rate remained constant, every third or fourth breath seemed deeper than the others. This irregular rhythm is well shown in a tracing, Fig. 12, Plate I., taken from an animal which had been breathing ether for a time, and in which death was being induced by chloroform. It is further seen in Fig. 13, Plate I., taken from an animal also dying under concentrated chloroform, and in which the left vagus had been cut.

But the characteristic features of this respiratory rhythm come out in a record obtained by means of the water manometer to which I have referred. This apparatus possesses the advantage over the tambour that its indications are absolutely quantitative, so that we have the value of each respiration accurately recorded.

Further, it gives very delicate indications of low pressure, and can thus be employed while interfering as little as possible with the normal respiratory currents. On turning to a tracing obtained by this instrument, Fig. 43, Plate IV., it will be seen that so long as air is being breathed the volume of each respiration is identical—that when chloroform is given the movements gradually and regularly increase in volume, until a maximum is reached, but that when succeeding this the movements begin to diminish in volume, this diminution is not a simple gradation, but consists of a succession of waves of increase and diminution, though the total volume is undergoing absolute decrease. The result is that we have a succession of phases, each of which may reasonably be compared to a Cheyne-Stokes' phenomenon, consisting, as it does, of a succession of waves leading up to a maximum, followed by another succession leading down to a minimum. The existence of these phases of the second stage are also beautifully shown in the tracing Fig. 44, Plate IV., obtained under similar conditions to the others.

It seems somewhat difficult to account for this remarkable rhythmic variation, unless it be that these larger waves represent the normal discharges of the respiratory centre appearing coincidently with the chloroform acceleration, and thus asserting themselves in this modification of the narcosis respiration.

2nd, During early anæsthesia, while consciousness is entirely abolished, the action of the vagus is distinct and easily demonstrated. Stimulation of either the trunk or the divided central end results in inhibition or quickening of the respiration, according to circumstances. Further, during the first stage of increased volume rate (the result of a lethal dose) this activity remains, though in a gradually diminishing degree; but on the accession of the second stage its action is abolished, and when the stage is fairly developed the strongest currents fail to produce any response through this nerve. This is well shown in Figs. 9–11, Plate I., and Fig. 45, Plate V. In the first of these tracings, Figs. 9–11, Plate I., the animal, after being anæsthetized, was allowed to breathe pure air for some time. A weak current (20) was thrown into the left vagus, and almost complete respiratory cessation followed (Fig. 9). Chloroform was given at the point *Ch.*, Fig. 10, and still the same effect follows the same current a few records afterwards. Fifteen seconds elapse between 10 and 11, and now it is seen that little or no effect follows the introduction of the same strength of current, and even very strong currents seem, a little later on, to produce but the faintest impression. Fig. 45, Plate V. shows a similar result. The explanation of this seems to be that in the early stage the excito-respiratory effect may be due to the action of chloroform on the pulmonary terminations of the pneumogastric, during which period these stimuli are carried to a centre capable of being affected by them while in the later stage, the phenomena of which are due to the

paralysis of the centre; afferent currents, however strong, fail to produce any effect whatever.

3rd, A point of some practical interest deserves notice. How will the final result be affected by the withdrawal of the vapour during any of these stages? How far may the chloroform be continued with safety? Numerous observations have shown me that the withdrawal of the vapour during the early stage (*i.e.*, before the respirations have reached their maximum) is usually followed by recovery of the normal rhythm after a time, but, on the other hand, the continuance of the administration into the second stage, and its withdrawal after diminishing volume appears, is not followed by such recovery, but that the gradually diminishing volume continues until it is finally extinguished.

II. *Action of Concentrated Vapour of Chloroform given Intermittently.*

If an animal be kept under the influence of the vapour for a lengthened period, by its frequent administration and withdrawal, we find that in the early part of the experiment the characteristic quickening of the respiration will follow each dose, and that the return to the normal rhythm will gradually succeed its withdrawal. Later on, however, certain remarkable phenomena appear, with sufficient frequency to warrant us in attributing them to the special way in which the vapour is given.

1. Death may occur in one of several ways.
   - *a.* In some few cases where the experiment has lasted less than two hours, the mode of death is similar to that described under the previous heading.
   - *b.* In a much larger number of cases the respiration ceases with a marked slowing and irregularity of the waves. This is well shown in the case of an animal which had been under the influence of intermittent doses of the concentrated vapour for upwards of three hours, and in which the cessation rate produced by the final dose was in successive periods of 10 seconds—0–10, 21; 10–20 21; 20–30, 21; 30–40, 20; 40–50, 20; 50–60, 19; 60–70 17; 80–90, 15; and the final curves are exceedingly irregular, as seen in Fig. 22, Plate II.
   - *c.* In the third place, death may occur during one of the administrations with remarkable suddenness. I have frequently observed that after an animal had been breathing air for a period of, say, ten minutes, and when the vapour was being reapplied, heaving movements of the chest occurred and then suddenly ceased, the respirations immediately preceding such cessation being quite deep and full. This form of cessation is well seen in Fig. 46, Plate V.

2. Besides these variations in the mode of death, I have observed a remarkable condition exhibited by rabbits under the intermittent concentrated vapour, which I venture to think worthy of notice, from a practical as well as theoretical point of view. I do not venture to speak with entire confidence as to the explanation of the condition, as I have not been able to reproduce it sufficiently often to enable me to arrive at a definite conclusion regarding it. As the practical bearing of the point it raises is considerable, I have thought I might be justified in bringing it under notice, though as yet inadequately investigated, in the hope that it may receive some attention from those in circumstances to study it.

It frequently occurs that when a rabbit has been kept for a considerable time under the intermittent administration of chloroform the respiratory volume changes with great suddenness, and from being deep and regular becomes shallow and wavy. Examples of this will be seen in Figs. 16 and 25, Plate II. On the other hand, the converse may occur, and the volume suddenly increase. Moreover, these changes occur without any apparent increase or variation in the extent of the chest and abdominal movements, so that a light lever laid on these regions does not show any sign of the alterations referred to.

But further, I have now tracings from six cases, in which, with little or no previous alterations in rate or rhythm, the air current, as indicated by the recorder, comes to a sudden standstill, the pen drawing a straight line. But while this was so, the movements of the chest still continued, neither their rate nor their extent being appreciably altered. Such a state of matters may last for an uncertain period, but is usually ended by the air current recommencing in the tube. These tracings are reproduced in Figs. 14 to 21, Plate II.

It will be observed that the line indicating absence of the air current continues for a considerable length of time,—16, 44, 21, 32, and 28 seconds in five cases recorded.

It is further to be observed that the respirations immediately before and immediately after the cessation of the air current are not specially remarkable. In some they are slightly exaggerated, in others slightly irregular. In all those referred to, the animal had been some hours under chloroform. In the sixth example of this nature, Figs. 19 to 21, Plate II., the tracing is altogether remarkable. The animal, a full grown male, had been under chloroform for 2¾ hours, and respiration had ceased under its influence. Resuscitation was effected by artificial respiration, and immediately thereafter the recorder was connected and the tracing Fig. 19 taken. It will be noted that the line traced by the pen shows distinct though shallow respiratory waves, indicating a certain air current; at *a* the tube was momentarily pinched, with the result of producing a sudden crest of considerable height. This is followed by six respirations of ordinary depth, then it sinks again to the previous

shallow state. At *b* the tube was again pinched with a similar result, and again at *c*—this time only a deeper wave being produced. 13 seconds after this, however (at *d*), 7 deep respirations occur spontaneously, followed by a considerable number of average respirations. Again the shallow type supervenes, and then again is followed by a succession of greatly exaggerated waves. Now, during all these various changes in the recorded movements it is to be noted that the rate is absolutely the same, 9½ crests in 10 seconds. And further, it was observed at the time that the respiratory movements in the chest and abdomen were as distinct during the occurrence of the shallow breathing as during those showing normal volume, and that these movements were but slightly increased during the very deep waves which appear from time to time.

Now, this cessation of the air current, seen as occurring totally in the first five observations referred to, and its modification in this remarkable way in the last one described, can only be accounted for, so far as I can see, in one of two ways.

1. Either a plug of some sort formed in some portion of the apparatus so as to cut off the air from the recorder; or,

2. It must be accounted for by some alteration in the action of the breathing apparatus, by which, while still continuing to move, it failed to effect the regular inflation and compression of the lungs, so that the air column came to a standstill—that, in fact, while the thoracic walls seemed to be performing their natural movements, no air was passing out and in.

Now, I believe I am justified in excluding the plug as an explanation of these tracings on the following grounds:—

1. Dipping the chloroform tube under the level of the liquid was followed by merely an almost imperceptible rise of the fluid in the tube. This showed that the plug was not simply in the tambour tube, there being no current in the main tube.

2. The tracheal tube was made of glass, and any plug or obstruction there could be seen.

3. Had the air current been cut off by a plug while the normal movements continued, it is obvious that the latter would have been much exaggerated from the dyspnœa induced.

4. The mechanical closure of the air tube for even a few seconds in an animal breathing in the ordinary way is always followed by violent thoracic movements which at once attract attention.

It has long been recognised by surgeons that thoracic and abdominal movements are no proof that air is entering the chest during anæsthesia, and an administrator of chloroform always assures himself of this fact by actually feeling or hearing the air current. The stoppage of the air current with continuation of movements of the abdominal and thoracic walls is usually explained in the human subject by the tongue falling back and the glottis

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in the course of 5 to 10 minutes by a slowing of the respiration, and by a peculiar jerky movement of the diaphragm observed by its effects on the viscera when the abdomen was opened. This occurred so distinctly in several cases that I took a tracing of the respiration in the ordinary way in the case of a full-grown rabbit, whose jugular had been opened, and which had lost a good deal of blood. The tracing, as shown in Figs. 23–25, Plate II., is sufficiently characteristic to be worthy of notice. Fig. 23 shows the effect 15 seconds after the blood was withdrawn; Fig. 24, 1½ minutes thereafter; and Fig. 25, 5 minutes after Fig. 24. The first line shows the commencement of a peculiar wavy respiration. This is, however, seen to a very much more remarkable extent in the second line, where the breathing is much slower, and in which the notches of the curves are repeated with great exactness. In the third line the rate is again rising, the notched character of the waves still being present. At a certain point the volume suddenly falls, the rate remaining much the same. This curious alteration in the nature of the curves is interesting in connexion with another tracing, to which I shall direct attention immediately. Whether it has any practical bearing on the effect of hæmorrhage during operations I cannot venture to say. In rabbits it certainly is an almost constant sequence of anything like serious loss of blood. It would be of much interest to know whether any such alteration of the respiration has been observed by surgeons in operations involving the loss of much blood.

It seems to suggest the question as to how far the respiratory centre depends for its rhythmic action on a certain *vascular* tension in the brain. May it not be that the diminution of the tension following the loss of blood results in an interference with the rhythmic discharges from the centre, and hence the secondary waves seen in these curves?

As will be seen, the records give no account of the pulse rate or blood pressure during the respiratory changes.

The points of chief interest in such a relation are:—1. The changes in the blood pressure during the narcosis; 2. The relative moment of cessation of cardiac action and respiration.

The first point has been investigated with the greatest fulness (as I have already said) by both committees referred to, by Knoll,[1] Kratschmer,[2] and others, and nothing further seems to be done in that direction.

With reference to the second matter, the relative moment of cessation of the two movements, opinions do not quite coincide, but all are agreed that the stoppage of the heart before the cessation of respiration is a comparatively rare event in animals. The Committee of the Medico-Chirurgical Society[3] determined the moment of cardiac stoppage sometimes by means of the movements of the kymograph, and in other cases by means of the movements of

[1] *Loc. cit.* [2] *Sitzungb. Wiener Akad.*, Oct. 1870. [3] *Loc. cit.*

a needle pushed into the tissue of the heart through the thoracic wall.

They mention four cases in the report in which the heart stopped first, and this happened out of many experiments. The Glasgow Committee[1] had only one animal whose heart stopped before the respiration, and this was out of nearly a hundred experiments.

In none of my experiments on nearly fifty animals has the pulsation in the carotid stopped before the respiration. Usually it continued 1 to 2 minutes after, in some cases as long as 4 minutes.

In my earlier experiments I determined the moment of stoppage of the pulse by holding the vessel between the thumb and forefinger, but in my later observations I have employed an instrument which I have devised for recording the pulsation in unopened and unligatured vessels, which records have not, as far as I know, been previously obtained.

By this instrument, which I have termed the "Arteriograph," we are able to detect and record the faintest pulsation in the artery, without the trouble or source of error arising from the manometer.[2] The cutting off of part of the blood supply to the brain, following ligature of the carotid previous to connexion of the manometer, must be inevitably a source of fallacy with regard to investigations into the action of substances such as chloroform, etc., and I should venture to hope that in such investigations, where *pulse rate* is required, the instrument will prove of some use.

In all these cases in which I have employed it I have found that the cardiac movements continued a considerable time after the respiration had ceased—in the case of strong doses, beating vigorously; in the case of dilute doses, beating more feebly.

No doubt there are cases in which the heart gives in first, but they are very rare, and do not affect the results I have arrived at as to breathing, as all the various phases of cessation have occurred with the heart in action.

To summarize then, briefly, the results obtained by these experiments, I find that in rabbits already anæsthetised by chloroform respiration ceases, according to the mode of administration, in the following ways:—

A. The continued administration of the concentrated vapour.

1. Respiration shows three periods—
   - *a.* A latent period of 2-10 unaltered respirations.
   - *b.* A period of augmented volume and accelerated rate.
   - *c.* A period of diminishing volume and constant rate.
2. This last stage shows two sets of curves, the larger possibly due to the normal discharges of the centre

[1] *Loc. cit.*

[2] I must leave the description of this instrument and some of the results obtained by its means for another paper.

added on to the more rapid discharges caused by the chloroform.

3. During this stage the action of the vagus becomes gradually abolished.
4. Removal of the vapour during the stage of acceleration is usually followed by recovery, but in the last stage such removal does not affect the fatal issue.

B. The intermittent administration of the concentrated vapour. Respiration may cease in three different ways:—

1. In experiments under 2 hours the cessation occurs with diminishing volume and constant rate.
2. In others, the respiration slows somewhat towards the end, and shows great irregularity.
3. The respiration may come to an abrupt cessation, generally when the vapour has been given after air has been breathed for a long interval.
4. During the prolonged administration the air current may suddenly come to a standstill while the respiratory movements continue.

C. The continued administration of dilute vapour acts in a similar way to the strong, only the time occupied is longer.

D. The intermittent administration of the dilute vapour is followed by great slowing of the respiration towards the end.

E. Loss of blood produces an irregular "wavy" respiration.

F. The heart is affected after the respiration, and is less affected by concentrated doses administered continuously than by dilute doses given intermittently.

Restoration of Breathing after Respiratory Cessation.

Were death by chloroform much rarer than it unfortunately is, the study of resuscitation of subjects succumbing during its administration must be always one of deep interest.

The subject has been discussed at length in many papers, and fully treated in all works on anæsthetics, and I have no intention of entering on any consideration of the various methods which have been recommended or suggested to effect restoration of the breathing in those in which it has ceased.

Experiments on animals and accumulated experience on the human subject have gone to show that of all the means which have been recommended with this object artificial respiration is the most efficient. The importance of immediate resort to the method has been insisted on by Snow, Sansom, Anstie, Richardson, West, etc., the Committee of the Medico-Chirurgical Society, the Glasgow Committee, etc.

I need scarcely say that my own observations tend entirely in the same direction.

The method of carrying out such artificial respiration is usually one of three :—

1. Marshall Hall's, or Sylvester's.
2. Tracheotomy, and insufflation of lungs through the opening.
3. In simple cases, especially children, by mere compression of the chest, allowing it to fill by its own resiliency.

There is no doubt that a very considerable number of lives have been saved by the timeous adoption of one or other of these methods.

Unfortunately, however, cases occur from time to time in which the patient employment of one or all of these methods fails, and the cessation of respiration becomes permanent.

In the series of experiments on the uterus, to which I have alluded, and in which I was led to use chloroform as the anæsthetic instead of ether, from the efficiency of the former drug in subduing peristalsis of the uterine horn, my observations were frequently interrupted by the respiration ceasing owing to inadvertence in administering too much chloroform. Accordingly, I had frequently to practise artificial respiration with a view of resuscitating the animals. This was effected by simply blowing into the tracheal tube at intervals, and so distending the lung and allowing the air to be expelled by its collapse.

Such a process I found successful in perhaps two animals out of three. But if the same accident occurred twice in the same animal, resuscitation was, if successful a second time, effected only with great difficulty, and in most cases it entirely failed.

While performing the insufflation in the case of some of these animals, I was struck by the large quantity of the vapour which smell and taste showed was present in the air expelled from the chest at each recoil of the lungs. It was obvious that the saturation of the air in the lung cells with chloroform must be a serious bar to the re-establishment of respiration, and that the first indication was, therefore, to endeavour to get rid of this vapour as quickly as possible.

For, consider for a moment the sequence of events when we forcibly inflate the lungs of an animal which has stopped breathing under chloroform. As I have already stated, experience has shown that the heart almost invariably continues to act after respiration has come to a standstill. Moreover, in those cases in which the heart has already stopped it recommences its beat before the first resumption of spontaneous respiratory movements. Accordingly, the increase of tension in the lung following the first forcible inflation must inevitably result in the diffusion of another dose of the fatal vapour into the blood, which sweeps it on to the centre, and there extinguishes any beginnings of renewed action.

I venture to think that nothing more than this is needed to account for the "few spasmodic" gasps which we repeatedly read

about followed attempts at reanimation by Sylvester or Hall's method, in the account of fatal cases of chloroform poisoning. The statistics of the Medico-Chirurgical Society's Committee contains the record of many such cases. In such cases it seems to me likely that the movements of the lungs caused by the commencement of artificial respiration, mechanically or otherwise, excites the heart to somewhat more vigorous action. The respiratory centre is excited by the increased blood pressure, and a few discharges occur, but its activity is soon extinguished by the stream of blood now coming from the lungs laden with the deadly vapour, which has been forced into it in the way I have indicated. Accordingly, acting on this idea, in my next experiment I commenced attempts at resuscitation by sucking air out of the lung through the tube, and simply allowing the lung to fill with air by its own recoil. This process was aided by compressing the chest wall gently while sucking the air out. In the course of a few seconds the nostrils began to move, and after five or six such aspirations the natural breathing reappeared and soon passed into gentle respiration. After a little time the respiration was again stopped by means of chloroform, and again resuscitation was effected by a similar method—this time requiring a little longer for its accomplishment. The same process was repeated twice again, four times in all, on this animal—the last commenced 25 seconds after breathing had ceased, and occupied about 1½ minutes before breathing was fairly established.

Since that experiment I have made a large number of observations on this method, and certainly have found it most efficient. In only one case have I found it fail in adult rabbits, resuscitation being readily effected four, five, or six times, even after the animal has been upwards of two hours under the influence of the vapour, and in deep anæsthesia all the time.

The successful repetition of the experiment oftener than four or five times was, however, usually prevented by the air passages getting obstructed by a pink frothy exudation which developed in them, and which I attributed to the strain in the bloodvessels caused by the frequent exhaustion of the air cells. I never saw this appear until after the third or fourth time.

The desire to avoid this, however, caused me to hit upon the following modification, which I have employed recently and with greater success than simple inflation. This process, which I may term "perflation," I carry out in the following way:—Disconnecting the rubber tubing I take the end of the T tube attached to the tracheal cannula in my mouth, and closing the branch with the finger, I make one or two aspirations of the lungs, compressing the chest gently at the same time. This removes a considerable quantity of vapour from the upper passages. Then opening the branch I make a series of deep inspirations. The air rushes in by the branch, and no doubt the greater part passes into the mouth.

Yet some of it enters the lung, and a current is thus established by which a very large quantity of the chloroform is rapidly expelled, as can be proved by the taste of the air coming through the tube. After two or three such inspirations the taste of the vapour becomes fainter, and as soon as this is noticed I reverse the process, now blowing air into the tube, with force just sufficient to cause the chest wall to move in the slightest possible degree—the branch tube being open all the time. Generally, after one or two such perflations the heart shows signs of vigorous action, and shortly thereafter breathing commences and continues in a perfectly natural manner. Should it not return so rapidly, and after I am assured by the absence of taste or smell in the expired air that the chloroform has been almost entirely removed, then I close the branch tube and commence gentle inflation of the lung in the ordinary way.

By such a method I have resuscitated animals as often as ten times after cessation of respiration, and in these I have little or no indication of the production of the pink froth to which I referred.

Of course, in order to accomplish resuscitation so frequently as eight or ten times it is necessary in the later stages to lose no time between cessation of movement and commencement of the effort, but the mere fact that it can be effected under any conditions so frequently speaks strongly, I think, for the efficacy of the method. In the earlier resuscitations we may safely wait 30 to 60 seconds before beginning the attempt, provided the animal has not been for a long period under the influence of the vapour. The time occupied in the operation is much less when we begin before the heart has ceased to beat, but I have repeatedly waited until all indications of the heart's action had ceased, both as indicated by the apex beat and the pulsation in the carotid. But in such cases it has been an invariable experience that the heart resumed beating a considerable time before breathing commenced.

Speaking broadly, as regards the difficulty of resuscitation as indicated by the time required to effect it, I have observed that *the time required to restore respiration varies inversely as the concentration of the dose, and directly as the time required to stop respiration.* That is to say, the more concentrated the dose the easier was the reanimation, and the longer respiration continued under the action of the vapour the more difficult was the reanimation. This observation corresponds with that made on the same point by the Committee of the Medico-Chirurgical Society.

Thus the greatest difficulty was experienced in reanimating animals which had succumbed under the long action of dilute vapour intermittently administered, and in which respiration ceased with great slowing of the rate, as in Figs. 26–32. This

appears to me to be explained by the fact that chloroform is primarily a respiratory poison, and that, if given in sufficient quantity, it stops the action of the respiratory centre before it affects the heart. Accordingly, the heart may be beating vigorously some time after the respiration has ceased under the concentrated vapour. On the other hand, the prolonged circulation of the vapour in the blood ultimately affects the cardiac action, and, consequently, under dilute chloroform, the heart may stop with or immediately after the respiration, and hence the difficulty of resuscitation.

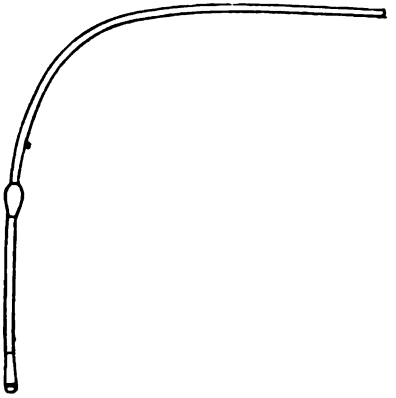
To show how well the characters of respiratory cessation under concentrated vapour which I have described are retained, even after repeated reanimation, I show tracings of the final stages of four cessations in the same animal, Figs. 5, 6, 7, 8, Plate I.; where it will be seen that it is only in the last one, when the animal had succumbed four times, and been more or less under the vapour for 4½ hours, that any slowing is observed, and even then it is not great. So also in Fig. 47, Plate V., taken after seven resuscitations, we have a similar correspondence with the mode of death occurring under the earlier administrations.

The observations recorded in this communication touch more than once on points of practical interest, but which time will not permit me to allude to. I content myself at present with a reference to one matter only, namely, that of resuscitation after respiratory cessation.

It is with diffidence that I venture to offer any suggestion whatever on so grave a matter as the method of re-establishing respiration in such cases, and this all the more, when the suggestion refers to a mode somewhat different from that ordinarily employed and sanctioned by experience. But the fact remains that the ordinary method sometimes fails in its object, which failure may, in certain cases at least, be accounted for by a defect which I have indicated. Encouraged, then, by the success of the experiments on animals peculiarly susceptible to the action of chloroform, which has followed the method I have described, and convinced of the physiological soundness of the principles involved in it, I would venture to suggest that in any case of respiratory cessation under chloroform (with the heart beating after the respiration had ceased), and where the removal of the vapour from the face, the drawing forward of the chin, and compression of the chest have failed to restore breathing, that the method of "perflation" should be tried without delay.

It might be done in lieu of a better means by a gum elastic tracheal tube as figured, provided with a conical collar to make it fit tightly into the glottis and prevent it slipping too far in. This could be slipped over the tongue and into the glottis, and the collar pressed down into the rima. The mouth is applied to the tube and several deep inspirations made. The mouth is with-

drawn from the tube after each inspiration, in order to let the lungs of the patient fill with pure air by the recoil. When this has been



repeated several times, the tube might then be slightly withdrawn so as to separate the collar from the glottis, and the process of perflation commenced by making deep and forcible inspirations. Air will rush into the trachea by the side of the tube, a current will be established, which will tend to carry air into the lung and sweep out the vapour. So long as any distinct odour of chloroform accompanies the air withdrawn the process should be continued, provided the natural respiration does not return. When all indications of the presence of the vapour in the lung have disappeared, then the inflation of the lung may be commenced, either by forcing air in through the tube, or by Marshall Hall or Sylvester's method. I should expect that most frequently the natural efforts would show themselves before this was necessary.

I regret that the "strong arm of the law" prevents me demonstrating the efficacy of this procedure in recovering animals from chloroform, and I must accordingly confine myself to a description and recommendation of the process. The advantage I claim for it is the avoidance of any increase in the tension in the air cells so long as chloroform is there, while providing a quick and efficient means of sweeping the vapour out of the lungs.

I feel that I cannot better demonstrate the importance of the removal of this vapour by some such means than by showing, in conclusion, a tracing which proves how completely the circulation of blood saturated with chloroform may paralyze the respiratory centre, Figs. 33–39, Plate III.

The subject was a full grown male rabbit, which had been under the influence of the vapour for 3 hours. Cessation occurred under the concentrated vapour with distinct slowing at 5.19. Carotid pulsation ceased at 5.21. Two minutes after, 5.23, resuscitation attempts commenced by blowing air into the chest at intervals, but without effect. At 5.25 the left jugular was opened, and blood

of a dark colour flowed freely. Fifteen seconds afterwards a gasping respiration commenced, consisting of a jerky inspiration followed by an expiration and succeeded by a long pause, Fig. 33. This reached a maximum depth in 20 seconds; at 56 seconds the interval is interrupted by a shallow notch (Fig. 34 *c*), which intermediate respiration gradually increases in depth, until, by-and-by, at 160th second, a wavy form of regular breathing is developed (Fig. 35 *d*). This continues for a few seconds, but soon begins to diminish in depth, until, at the 207th second, it is scarcely perceptible (Fig. 36 *e*). At the 212th second the gasping respiration again appears, passes through a similar period of increase and diminution, is succeeded again by wavy breathing, which again diminishes in volume, disappears for a time, and is succeeded by the gasping respiration, and so on; the cycle is repeated with longer intervals of complete cessation. Ten minutes after the conclusion of this tracing perflation was practised, and normal breathing established.

This curious record I would explain in this way:—The animal being a long time under chloroform, the cardiac force was much reduced. Cardiac cessation occurred with the walls exhausted and distended on the right side. Opening of the jugular relieved the pressure, the heart began to beat, the respiratory centre became excited, a few respirations followed; but the blood, in passing through the lungs, becomes freshly charged with chloroform, and, passing to the centre, again gradually paralyzes it. But as the respirations become fainter, less and less chloroform is being absorbed, so that, by the time they cease, the blood passing to the medulla contains little vapour; and the deoxidized state of the blood excites it to action, which results in the gasping respirations again appearing, only to be followed by a similar absorption of vapour, and consequent diminishing volume. On the other hand, the whole character is entirely altered by free perflation and consequent return of natural conditions.

EXPLANATION OF TRACINGS.

The tracings on Plates I., II., and III. are photolithographs from the originals, reduced one-half; those on Plates IV. and V. are facsimiles.

Plate I.

Figs. 1 *and* 2. Cessation of respiration under concentrated chloroform; animal for three hours previously under ether. There is an interval of five seconds between 1 and 2. Time line shows ½ seconds.

Fig. 3. Respiration during chloroform anæsthesia; the animal at this time breathing air only.

Fig. 4. Respiration under concentrated chloroform vapour; tracing commences ten seconds after administration of final dose.

Fig. 5. Cessation of respiration under concentrated vapour; commences thirty seconds after Fig. 4. Time line in these and all succeeding tracings shows seconds.

Figs. 6, 7, *and* 8 show successive respiratory cessations in the same animal, resuscitation having been effected by artificial respiration (see page 128).

Figs. 9, 10, *and* 11 illustrate the abolition of the action of the vagus during the later stage of chloroform narcosis.

Fig. 9. Anæsthetized, but breathing air.

Fig. 10. Chloroform given at (*Ch.*)

Fig. 11. Deep narcosis; vagus stimulation produces little or no change. The rise in the lines under the tracings shows the time during which the vagus was stimulated (see page 116).

Fig. 12. Peculiar irregular respiration induced by a lethal dose of chloroform after prolonged anæsthesia under ether.

Fig. 13. Similar breathing under a lethal dose of chloroform, the left vagus having been divided.

Plate II.

Figs. 14 *to* 21. Examples of cessation of air current, while the respiratory movements continue (see page 118 for full description).

Fig. 22. One mode of respiratory cessation following the prolonged intermittent administration of concentrated vapour (see page 117, II. *b*).

Figs. 23, 24, *and* 25. Effects on respiratory waves following extensive loss of blood.

Plate III.

Figs. 26 *and* 27 show cessation of respiration under prolonged action of the dilute vapour. A and C are continuous, taken by œsophageal tube; B and D continuous, taken by stethograph.

Figs. 28 *to* 32. Cessation of respiration under conditions similar to the former tracings; the animal had been under dilute vapour for three hours previously. Final dose given at (*Ch.*)

Figs. 33 *to* 39. Resumption of respiration following bleeding from jugular vein (for full description see page 129).

Plate IV.

Figs. 40 *and* 41. Cessation of respiration under concentrated chloroform vapour, both from same animal, resuscitation having been effected after 40. Final dose of chloroform given at (*Ch.*)

Fig. 42. Tracing obtained under similar conditions to the last, taken by water manometer. The animal was a half-grown female.

Figs. 43 *and* 44. Cessation under concentrated vapour; animals full-grown males. Tracing taken by water manometer. Shows double waves in the later stages (see pages 115 and 116).

Plate V.

Fig. 45. Effect of chloroform on the action of the vagus (see page 116).

Fig. 46. One mode of respiratory cessation after animal has been under the influence of repeated doses of the concentrated vapour (see page 117, II. *c*).

Fig. 47. Respiratory cessation in an animal which had been resuscitated seven times by the method described (see page 128).

Fig. 48. Respiratory cessation in a rabbit rendered unconscious by 15 grains of chloral hydrate given two hours previously. Chloroform given at (*Ch.*)

Dr Smith, President of the Royal College of Surgeons, said he had much pleasure in complying with the President's request that he should open the discussion, although he felt less competent to deal with the novel and deep questions involved in the able paper just read than some of the physiologists whom he saw present. He would have felt more at home had the discussion promised to be of a more practical nature, and dealing more with anæsthetics in a surgical than in a physiological point of view. What had been stated in the communication, to which they had listened with so much interest, was of great importance, but would require much time and careful consideration before justice could be done to it in a discussion of this kind. From the respiratory tracings shown, it appeared to him that towards the close of the protracted exhibition of chloroform, in the experiments cited, something occurred to produce the irregular and feeble markings at that time, which was analogous to the conditions resulting when all *sensory* nervous action, as conveying stimulation to respiratory action, was destroyed, but where in such cases there still continued an irregular and shallow respiration, possibly due to the circulation of unoxygenated blood through the inspiratory centres, and of blood loaded with carbonic acid through the expiratory centres on the sides of the medulla. With regard to death from the arrest of respiration in the human subject, although he did not in the least doubt that such an effect was producible where chloroform was pushed to the extent to which it was carried in the experiments detailed, he suspected that a number of the deaths recorded as due to chloroform in surgical operations might be attributed to other causes. He remembered cases of death on the operating table being of not uncommon occurrence before the introduction of chloroform at all, and he thought the President himself would bear out this statement. The number of deaths from surgical operations largely and suddenly diminished on the introduction of chloroform, as was shown by Simpson's remarks on Dr Philip's statements in the *Medical Gazette* for 1844 or 45, as well as in a number of able and exhaustive arguments to the same effect. Death in such experiments as those described might be said to be from *poisoning* by chloroform pushed to its fatal toxic power. But the cases of death *under*, though perhaps not *owing* to its influence in the human subject, were of a somewhat different nature. Had this agent been

of so deadly a description as it was sometimes tried to be made out to be, it would have increased the mortality after surgical operations instead of reducing it, as it had done, to half what it had formerly been. Dr Smith adduced a number of cases where, had chloroform been administered, death would infallibly have been attributed to its influence. He thought that the paper read was interesting and important in its bearing upon what may possibly be the action of various substances upon the peripheric terminations of the vagus, both as affecting the lungs and heart, and the dissimilar results which might thus be produced on the circulation in the one case and the respiration in the other, was a consideration which would be of not a little practical value.

Mr Joseph Bell would not think of venturing to discuss the physiological side of the paper without having first made a careful perusal of it, and perhaps not even then. One practical point he, however, thought worthy of mention, and that was, that the administrator of chloroform should see that the patient was not allowed to over-chloroform himself. This frequently happened in cases where the anæsthetic was not inhaled with regularity, but with considerable effort at first, and was a cause of very great danger. The perflation idea was an admirable one. One point at least would be attained by it. The glottis would be kept open. Only the other day he had almost had a death from this cause, the glottis being allowed to close and the epiglottis to fall back during the administration of chloroform. When the glottis was opened, the patient respired naturally.

Dr James said it was usually a matter of regret that lateness of the hour prevented the proper discussion of many of the papers brought before them. In this instance there was less cause for it, inasmuch as the paper was one that required digestion before anything in the way of criticism was said regarding it; but as Dr Murray had devoted so much care and trouble to it, he was entitled to any remarks that members could give on points that had struck them. One point had struck him on which he would like to get a little more information. Dr Murray had referred to instances in which the chest walls appeared to heave, but in which the water-manometer recorded no ingress or egress of the air, showing that though the chest-walls were moving, the lungs were not being filled with air. He thought this was due to the disturbance of the innervation of the parts. It was what they sometimes met with in auscultating. They asked a patient to take a long breath. He immediately made a great inspiratory movement, but no air entered the chest. Any one could bring about this condition in himself if he attempted to take a full inspiration and at the same time contracted his abdominal muscles. He thought it was probable that the abdominal muscles were made to contract during the administration of chloroform, so that the abdominal viscera filled up the space which should be occupied by

the air. The jerking respiration associated with loss of blood might be explained in a similar way. In auscultating one got frequent examples of jerking respiration, and the causes for this were very various. Catarrh of the apex was one. The changes in the size of the lungs brought about by the action of the heart was another. There were other causes; but this interference with the innervation of the part, if it did not explain all Dr Murray's cases, was nevertheless one which should not be lost sight of. As regards the treatment, there was no doubt that from a physiological point of view the method indicated by Dr Murray was correct. Chloroform had got into the blood, and it was important to get it out. Drawing it out by aspiration was the best way. Aspiration would not only empty the air-cells of the air charged with chloroform, but might even draw the chloroform out of the blood. It struck him, however, that in cessation of respiration from other causes another mode of treatment must be employed. There is not something in the blood that should be out, but something in the air that should be in. In such a case, inflation would be better.

Professor Chiene was much interested in the attributing of the cessation of the respiration to a stammering condition of the respiratory muscles—the point to which Dr James had alluded. There was no doubt that in the human subject this stammering was one cause of the cessation of breathing. Another cause was undoubtedly the falling back of the tongue and epiglottis, and the closure of the glottis. He had learned the value of the insertion of the finger, as practised by Mr Bell, which acted reflexly as well as mechanically in opening the glottis. This and Mr Lister's explanation, that the drawing forward of the tongue acted reflexly in checking the stammering respiration, agreed with Dr Murray's observations and deductions. As to the practical points which had been noticed, he was certain of the truth of Mr Bell's remark, that after a person had got a quantity of chloroform he might, so to speak, go on for some time chloroforming himself from what was in the air in his chest, though the towel or other apparatus was away from him. Their object in such cases, then, should be to get the chloroform out of the chest; and he thought practical good would come of Dr Murray's suggestion.

Dr Allan Jamieson was struck by the slowing of the respiration caused by loss of blood. There was a condition analogous to this in anæmia and spanæmia, in which there was a peculiar slowing of the respiration. Apparently in these states there was a want of irritability of the respiratory centre, so that breathing did not go on well under the administration of chloroform. Altered conditions of the blood could not perhaps be observed in rabbits, but their effects in the human subject were worthy of notice.

Dr Graham Brown referred to experiments made by a foreign physiologist, about six or eight years ago, who observed that if

chloroform was administered to dogs in large quantities, and pretty rapidly, there was a sudden inhibition of respiration. This inhibition was somewhat controlled by section of both superior laryngeal nerves, but not completely. If along with these the vagi were cut, there was a complete inhibition of the respiration. So far as the upper half of the tract was concerned, its inhibition was controlled by the superior laryngeal. The lower half was governed by the vagus or its recurrent laryngeal branch. In the later stages of chloroform narcosis, he fancied, one would have two causes for the cessation of the respiration,—either, what Dr Jamieson alluded to, the defective supply of blood to the respiratory centre, which they might conclude did occur, there being almost no blood pressure at all; and, second, that the blood was so saturated with chloroform that there was a local poisoning of the centre.

Dr MacGillivray had unfortunately, though not in his own practice, seen nine or ten deaths in London, Edinburgh, and abroad during the administration of chloroform. He had not the slightest doubt that most of these were cases of over-dose due to the fact that administrators had been taught to go on giving the chloroform till the conjunctiva was insensible. In one or two instances the deaths were almost certainly due to heart disease, and in another to asphyxia; but in the others there was undoubtedly an over-dose as the chief cause of death. There was another point of interest, and that was that whether from loss of blood or from shock due to prolongation of the operation, much less chloroform was required towards the end of a long operation than at the beginning. The most painful part—the insertion of the stitches—was at the end; yet the patient, though apparently out of the chloroform and quite conscious, very often felt no pain when this was being done. Perhaps Dr Murray might give an explanation of this peculiar phenomenon.

Dr T. A. G. Balfour said he had been of opinion that most of the troubles and deaths that occurred during the administration of chloroform were due to over-dose; that they were, in fact, cases of asphyxia, due to the exclusion of the oxygen of the atmosphere by the air-tubes being full of the vapour of chloroform and the blood being saturated with it, but a statement by Dr Murray led him to doubt this. It was that Dr Murray had found that the greatest danger was when he began to re-administer chloroform to animals to which for some time he had admitted only fresh air. He should like to have some explanation of this curious circumstance.

Dr Shand related a case in which, on the advice of the late Professor Alison, chloroform was given to prevent the daily recurrence, at a particular hour, of a nervous cry, which was supposed to be the result of a head injury. On the first day 2 ounces failed to produce anæsthesia. On the second, half an ounce succeeded, and in the course of time 15 minims were sufficient. Latterly, 5 minims alone were necessary, and the case was cured.

Mr Hare said the condition which Dr Murray happily called respiratory inco-ordination occurred in those cases in which he had been administering concentrated chloroform vapour intermittently. The vapour given to the human subject was diluted and uninterrupted. Did the inco-ordination happen in such conditions? In reference to the tube which Dr Murray suggested for removal of the chloroform vapour from the lungs, he wished to ask whether there was any provision in it to prevent the suction applied by the mouth being too great. It seemed to him that if not, a pulmonary hyperæmia, more particularly in children, might be caused.

Dr Murray thanked the Society for the kind reception it had given his "maiden effort" there. Mr Bell had referred to an interesting point—the possibility of allowing patients to over-chloroform themselves. He could confirm Mr Bell's opinion in the most absolute fashion as regards animals. If one was in a hurry, and screwed the animal to the clamp before sensori-reflex was quite abolished, it sometimes commenced to breathe with extreme violence; and if the chloroform bottle was connected with the tube, the animal was dead in a few seconds. Dr James's suggestion regarding the cause of the stammering bore in a direct way on the point, and he was glad to have this clinical confirmation of his observations. As to asphyxia, he agreed with Dr James that the process he recommended could not be applied. In reference to Professor Chiene's remark about reflex stimulation, he (Dr Murray) had omitted a point to which he should have referred. In one case of stammering, while the chest movements continued the tambour stopped. The conjunctiva was pinched, whereupon the tambour immediately indicated an air current; and this was repeated several times. It seemed to him quite likely that the pulling forward of the tongue acted, as the pinching of the conjunctiva, in a reflex manner. Dr Jamieson's remarks reminded him that he had often experienced great risk in the giving of chloroform to cases of uterine fibroid in which there had been much loss of blood. In such cases the patient would often stop breathing for some time. Similar experiments to those referred to by Dr Graham Brown were, he thought, made by François-Franck, who had since found that in rabbits the same effects were produced by dividing the trigeminal. With reference to over-dosage, Dr Hare appeared to think that the respiratory stammering was not likely to occur in man, because the vapour was diluted. It seemed to him that they were not certain as to the amount of dilution of the vapour. It was quite possible that an over-dose of the concentrated vapour might be given at the commencement of an administration, and whenever fresh quantities were poured on the cloth. In cases of ovariotomy, it was well known that the patient might suffer no pain even when the hand was in the abdomen, and yet be practically conscious. In the recent discussion on anæsthetics at the Medical Society of London, Mr Braine

referred to this fact, stating that he had several times had patients capable of answering questions entirely unaware of the fact that the operator's hand was at that moment in the abdomen breaking down adhesions. The danger, then, was that when a fresh dose had to be given to permit of the stitches being put in, the deep inspirations which followed might cause the inhalation of a quantity of vapour fatal to the patient so long under the influence of the anæsthetic, and cause sudden stoppage of the respiratory movements in the way shown in some of the experiments.

Meeting VII.—April 15, 1885.

Dr H. D. Littlejohn, *President, in the Chair.*

I. Exhibition of Patient.

Dr Carmichael showed a girl, æt. 3 years, suffering from FACIAL PARALYSIS AND DOUBLE CONVERGENT STRABISMUS. The child had been under treatment in the Royal Hospital for Sick Children. On examination she was found to be ill-nourished, and suffered from a double convergent strabismus, some vertical nystagmus, and facial paralysis of the left side. There was no previous history of disease beyond an attack of whooping-cough. The case had been under observation for several weeks, and they had not been able to throw any light on its true pathology, further than that they believed that the lesion was localized in the nerve centre. There was no appearance of the palate or tongue being affected, and neither the history nor the symptoms pointed to the ordinary causes of facial paralysis, such as cold or rheumatism. The history and symptoms indicated the probability of a central lesion. What the exact nature of this was, he was not prepared to say. There might be some tubercular lesion in the medulla, or some local changes due to syphilis. An interesting point was the double paralysis, the left portio dura and both sixth nerves being affected. The nuclei of the sixth and seventh pairs were closely connected at their origins, and it was not unusual in cases of central lesion to have one abducens and portio dura affected, but paralysis of both sixth nerves along with the seventh was rare. The treatment had consisted of nourishing diet and iodide of potassium, and she had improved to some extent on it.

II. Exhibition of Pathological Specimen.

Dr Mackenzie Johnston showed a THORACIC TUMOUR. The patient, a man, æt. 44, from whom the tumour was taken, consulted him first on the 12th August of last year. He complained of a change

in his voice and great difficulty of breathing. The tone of the voice gave one the impression that there might be some internal pressure causing paralysis of the laryngeal muscles. The chest was therefore examined, but beyond some dulness on both sides of the sternum, there did not appear to be anything abnormal. The larynx was afterwards examined by Dr M'Bride, who found that he had paralysis of both abductors of the vocal cords. The patient was then put on iodide of potassium, and told that he had a tumour or aneurism in his chest, probably the latter. He was not seen again until about a fortnight ago, when Dr Johnston was sent for to his house. He had great difficulty in swallowing, as well as acute bronchitis, which very markedly aggravated the dyspnœa, but on examining the chest the previous area of dulness was found to be much increased. The râles over the chest, especially on the left side, prevented the heart sounds from being examined minutely, but no murmur could be made out. The patient died next morning. At the post-mortem examination, which was made by Dr Bruce, the lung on the right side was found partially collapsed, and the upper part of that side of the thorax showed a tumour which at first was partly concealed by the heart and pericardial vessels. It was pricked with the knife, and a tremendous amount of fluid and broken down debris escaped. It was not, however, a cyst. A considerable solid portion remained. This, when removed, weighed about two pounds, and the fluid and debris which escaped must have weighed about one pound more. It was found to be sarcomatous. It was curious how so large a tumour could have existed so long without more symptoms. The patient had attended at his office up to about a week before his death, and the only symptoms he complained of were occasional dyspnœa, some difficulty in swallowing, and a change in the tone of his voice. Death had probably been caused by paralysis of the heart, some of its nervous structures being pressed on.

III. Exhibition of New Surgical Appliance.

Dr J. Moir showed some INDIA-RUBBER PADS FOR THE TREATMENT OF FLAT-FOOT. The steel supports which were usually inserted in soles of the patient's boots, though very efficient, got rusted and were sometimes broken. After trying two or three substitutes, he got some india-rubber pads which answered the purpose very well, but the difficulty was to keep them fixed. This was overcome by covering the under surface and side with cloth, which was sewn to the inside of the boot. He had now used them for several years and was thoroughly satisfied with them. Their advantages were that they were pliant, elastic, and yielded to the movements of the foot. They were durable, might be worn in different boots, and they were cheap. These pads are manufactured at the North British Rubber Works, Castle Mills, where much pains have been taken to make them as perfect as possible.

IV. Original Communications.

1. — OVARIOTOMY IN A COUNTY HOSPITAL, WITH A NOTE ON A METHOD OF PREPARATION OF CYSTS FOR EXHIBITION.

By Roderick Maclaren, M.D., Carlisle, Senior Surgeon to the Cumberland Infirmary.

For the sake of a short title to this paper I have used the word Ovariotomy to include more than its strict signification would justify. The cases to which I am going to ask your attention are of four kinds:—1st, removal of ovarian cysts; 2nd, of a Fallopian tube distended with purulent fluid; 3rd, removal of a non-cystic ovary; and 4th, exploratory or incomplete operations. They only amount in all to eleven,[1] and were it not that the undertaking of such operations in county hospitals involves some important considerations, and that several of the cases show points of some interest, I should hardly have ventured to take up your time in listening to the story of them.

For the removal of ovarian cysts I have had six operations. The patients have all recovered. Their ages were 50, 30, 49, 45, 61, and 64.

The first case had been tapped three times. The cysts were numerous, and contained altogether five gallons of fluid.

The second had not been tapped. Cyst shown.

The third had been once tapped. Cyst shown.

The fourth had never been tapped. Cyst shown.

The fifth had never been tapped. Cyst shown.

The sixth had never been tapped. Both ovaries cystic. Shown.

Ether was the anæsthetic used, except in one case.

None of the actual operations presented any remarkable difficulties. The first tumour was adherent to the abdominal wall and omentum, and the small cysts were so numerous that the incision had to be extended two inches above the umbilicus. The second was non-adherent but bulky. The third and fifth were extremely easy of removal, the cysts being non-adherent and capable of reduction to a small size. The fourth was much adherent, and, when emptied, still of great bulk. In the sixth case the left ovary was easy of removal, but the right, being very small and adherent, was rather troublesome to fish out.

The pedicles of the first two were clamped and retained outside the abdomen. The pedicles of the others were first cauterized and then tied with carbolized catgut: in two, the whole being tied

[1] Three other operations have occurred in my hospital practice since reading this paper. One a very large parovarian cyst with both ovaries, as they had early cystic disease. Two removals of small ovaries and tubes for chronic inflammatory disease. One recovered, and one died from peritonitis.

in sections; in the other two, only the most vascular portion being ligatured. The pedicle of the right ovary in the sixth case was cauterized, not tied. The operations were all done under spray and dressed with gauze, except the first.

I will now mention such peculiarities in each case as seem noteworthy. In Case I. the other ovary, the left, was converted into a cyst of the size of a filbert. I punctured, emptied it, and dropped it into the abdomen. It gave no further trouble. I saw the patient two or three years after the operation. There had been no enlargement of this ovary, and she was in perfect health.

Case II. was three months pregnant at the time of operation, and was delivered of a male child at the full time. The only inconvenience caused by the combined pregnancy and operation was some dragging in the pedicle as the uterus rose in the abdomen.

Case III. was the subject of an accident. The wound had been united by six sutures,—three deep of silver, and three superficial of catgut. On the sixth day after the operation the wound was perfectly united and everything well, so I removed all the deep sutures and one superficial one. She slept well on the following night, but at six in the morning had a troublesome coughing fit, and afterwards complained of considerable abdominal pain. In the afternoon it was noticed that the discharge was through the dressing, and when this was removed some coils of small intestine were found protruding from the wound. The catgut sutures still held, but were stretched by the bowel. The intestine was congested and swollen from constriction. The gauze dressing was sticking to it with much firmness. The bowel was washed with warm carbolic lotion and replaced in the abdomen. Two stout needles were passed through the skin and peritoneum, and the edges kept in contact with a figure-of-eight twist. The patient was a courageous woman, and allowed this to be done without an anæsthetic, and said it did not cause her much pain. For a few days she complained of some abdominal pain, but no febrile or other serious disturbance occurred. The needles were left in for eighteen days. She made an uninterrupted recovery, and was out of bed in a month from the date of the operation.

Case IV. was the only one in which I used a drainage tube. It was left in for twenty-four hours, and allowed several ounces of bloody serum to escape. I used it on account of the adhesions having been numerous. In the opposite broad ligament was a small parovarian cyst, which I ligatured and removed, and of which I show you a model.

Case V. was the subject of delusions of a melancholic type, her ideas running on numerous ailments to which she believed herself subject. She had frequent attacks of weeping, and altogether was very miserable. This mental state was uninfluenced by the removal of her tumour.

CASE VI., as already mentioned, was a double ovariotomy,—one tumour about twice the size of a newly born infant's head, the other about half the size of one's fist. During the six months before admission she had two attacks of abdominal inflammation, the second of great severity.

Under the head of distended Fallopian tubes I have one removal to report, a patient æt. 40. Her last confinement was eighteen months before admission to the hospital, and her statement is that her abdomen never resumed its natural size, and soon began to increase. For seven weeks she had suffered much pain. Her tumour reached 3½ inches above the umbilicus; it was dull to percussion, but the area of dulness was not always the same; the extent varied specially at the sides. Slight fluctuation could be felt. There was frequently crepitus on auscultation. The uterus was low and nearer the left than the right side of the pelvis. Downward pressure on the tumour moved the uterus, lateral movements did not. On operation the tumour was unusually adherent in front to the peritoneum, and elsewhere to the bowel and omentum. One portion of the cyst remained adherent to the bowel during the removal. I passed a ligature round it to mark its position, and separated it after the removal of the rest. The pedicle, which was small, was tied and dropped in. The cyst contained about 16 ounces of thin purulent fluid, which showed, under the microscope, pus cells containing much fat. Owing to adhesions I could not see the other tube and ovary, but they felt normal. She made a fairly rapid recovery, but has twice since had abscesses in the abdominal wall, the last time eighteen months after the operation. She is at present (twenty-one months after the operation) a patient in the Cumberland Infirmary, recovering from some deep abdominal inflammatory swelling. This operation, I may say, was peculiarly difficult.

Of the removal of non-cystic ovaries I have also one case to report. A patient, aged 26, married for a year. Her menstruation had always been irregular since its commencement in her sixteenth year. In four years she only menstruated six times, and not at all for eighteen months. She dated her illness from a strain four years ago, and her complaint was that her life was rendered miserable by pain situated mainly in the right side of the lower part of the abdomen, occasionally in the left. The pain varied in intensity, and when at its worst was frequently accompanied by vomiting. Sexual intercourse was very painful; vaginal examination elicited ovarian tenderness. She had been under many medical men; in fact, the note on her clinical card was—"previous medical attendants innumerable." She was sent to the Infirmary by Dr Macdonald, Kirkoswald, and was admitted under the care of my friend Dr Barnes as a medical case, and submitted to two months careful treatment without any advantage. As a result of a consultation of the staff, it was decided by a majority that an incision should be made, the ovaries examined and removed if diseased. The right

was found somewhat enlarged, one-third according to Henning's table; the surface was covered with small clear vesicles about the size of large pins' heads; there was no evidence that it had recently discharged any ovum; its Fallopian tube was intensely congested, and about the middle, on the upper surface, was a cyst the size of a small pea. I therefore removed this ovary and tube. The left ovary and tube seemed fairly healthy, and were left. At first the result was very satisfactory; after the first week the pain gradually subsided, and by the eighteenth day she described herself as perfectly well, and this improvement continued during her stay in the hospital, which extended to 2½ months after the operation, and during that time she menstruated twice. However, on her return to ordinary life the old symptoms came back, and she has expressed some anxiety to have the other ovary removed, as she now places the pain mainly on that side. I have hesitated to do this, because she also sometimes complains much of the right side.

Of exploratory and incomplete operations I have had three cases. In the first of these the operation was undertaken to determine the nature and, if possible, remove a very adherent abdominal tumour, which contained fluid. The patient, æt. 49, was known to have the tumour for twelve months; it was most prominent on the left side. Three months before operation it was aspirated, and 22 oz. of brownish gluey fluid removed. There was slight elongation and fixity of the uterus. When I cut down, by the usual incision, a cyst presented itself, which I opened. This proved to be a portion of the bladder which was drawn up to within two inches of the umbilicus. The pressure of the tumour cut off the upper part, giving the bladder an hour-glass shape. This upper part contained fluid, though a catheter had been passed immediately before operation, showing that the two cavities did not freely communicate. An examination with the hand in the abdomen showed the tumour to be malignant, and also that there were nodules in the peritoneum. The whole wound healed well, and she lived three years afterwards in much suffering, as the disease slowly invaded the neighbouring structures.

The second case, a woman, aged 53, was at first under the care of Dr Barnes as a medical case, and, after investigation, passed under my care. The shape, the area of dulness, and the palpation of the abdomen were such as to lead to the belief that the tumour was ovarian. One condition only raised a doubt—the area of resonance in the flanks altered slightly with position, but not more than some free fluid could account for. She was therefore aspirated. The fluid quickly reaccumulated, keeping throughout a central position. No abnormal condition of any organ could be detected, and as there seemed therefore every reason to believe the swelling to be ovarian, I arranged to open the abdomen. The case, however, proved to be one of ascites. From such examination as I could make by the incision I was not able to throw any certain

light on the cause. There was some trouble bringing forward a coil of intestine for inspection. Apparently the bowel was retained close to the spine by some diseased condition, probably mesenteric cancer, thus producing the physical signs of ovarian rather than ascitic dropsy. She did well after the operation for a time. But of course the fluid reaccumulated, and she was tapped seven weeks after, and again in a month. She subsequently left the hospital for her own home.

The third of these cases was the only one of all my cases which ended fatally. An unmarried woman of 33 was admitted for a tumour in the left iliac region; she had been ill for three weeks. On the evening of her admission I was sent for, our house surgeon, Dr Morton, believing her condition to be a grave one. She was suffering intense pain, vomiting frequently, having dysuria, and much depressed. It seemed to me that she had an inflamed distended Fallopian tube, and that no time should be lost in attempting to give her relief. On cutting down, the cyst was separated without much difficulty from the parts to which it was adherent in front, but in the pelvis the adhesions were too dense to allow of removal, and the pelvic portion was left. It was an intensely inflamed hydro-salpinx, and contained turbid serum, probably becoming purulent. Bleeding was troublesome, many vessels had to be tied, and it recurred during the night, necessitating the reopening of the wound. The operation was done by artificial light under spray, and presented many difficulties. Two drainage tubes were inserted. On the fourth day some fæcal matter exuded from the wound, and she died two days after from general peritonitis. The wound became septic on the third day. My suspicion is that in tying some vessel a portion of intestinal wall was included, whence the result. No damage was done to the intestine during the operation.

With the exception of the last, these operations were all done in a part of the hospital which has been in use for forty-five years, and in wards which, between the operations, were unavoidably used for all kinds of medical and surgical ailments, often of a very objectionable character. Eight were placed in wards which gave 3400 cubic feet to the patient and nurse. Two were placed together in a ward which gave 5400 cubic feet to them and one nurse. Eight of them had their own special nurse; three were under the care of nurses who had other patients. The last one, the fatal one, was done in a ward ten years old, and which contains eighteen patients—she had only the general nurse of the ward.

Miss Enderby, our late matron, has kindly given me a note of the precautions taken during her period of office. The chimney was stuffed with straw, the windows fastened up with strips of paper, the keyhole stuffed, and then the carbolic vaporizer was placed in the ward, and the door immediately closed and surrounded with strips of paper. If possible the ward was thus left undisturbed for forty-eight hours. Then the windows were thrown

open, the walls swept and washed with carbolic solution, also the bedstead and floor. The floor was finally polished. The mattresses, bolsters, and pillows, meanwhile had been through the process of disinfection in the disinfecting chamber (hot air), and, if weather permitted, were left exposed to the air in the grounds. Special blankets, sheets, surgeons and patients' towels, were marked for these operations, and kept on a shelf apart from any other. Before being put away, these articles of bedding were washed separately from any others in water containing carbolic acid, and hung out in the air to dry. A glass feeder was used. The nurses were cautioned to take a bath and to put on a complete set of clean clothes, and to take a walk before going to the operation. Either the sewing maid or Miss Enderby took charge while the day-nurse was at tea, and the serving maid cleaned the grate, so that when special nurses had charge no one who worked in the other wards was allowed to go in to the special ward for ten days or a fortnight. These precautions may be taken as typical of what has always been done, though they varied somewhat in detail. For instance, I have sometimes had the wards fumigated with burning sulphur instead of carbolic acid.

The sponges I always took to the hospital myself, first ensuring their cleanness by many washings, extending over three or four days.

Except the first case, all these operations have been done under spray. I have never had reason to attribute any harm to it. Often, but not always, the urine has been smoky for a few days, but I have not seen any other evidence of carbolic acid poisoning. And operating under the conditions I have described to you, it has seemed to me that what suited my other cases could not be an unnecessary precaution with these.

I may here just mention to you the other cases, apart from hernia, in which I have opened the abdominal cavity. Four times for local collections of pus, with one death. Two were round the cæcum, one in Douglas's pouch, and one in the left iliac region. Five times for intestinal obstruction, with four deaths—of these one was an endeavour to find a probable strangulation. One was an obstruction due to inflammatory swelling. The small bowel was opened, relief followed, and the patient is now alive. Another case, where the obstruction was inflammatory, was treated in the same way and died. With the two others enterotomy was performed, but they died, the post-mortem showing in the one case a twist, in the other an internal strangulation. In one instance the peritoneum was opened for the removal of a uterus, with a fatal result, and in another case two square inches of peritoneum were removed adherent to a sarcomatous tumour of the wall (tendon of external oblique). The patient never had a bad symptom, and had no return six months after.

The question has often been discussed whether operations of this

kind should be undertaken by general surgeons, or be only done by such as devote their attention mainly to this class of work. I do not purpose to enter at large into this question. As far as surgeons to provincial hospitals are concerned the matter stands thus,—If they do not consider themselves competent to include abdominal surgery in their work they must fail in an important part of their duty; if a man cannot or will not remove a simple cyst, what is to be done in the emergencies? Is he to refuse relief to every case that is unable or unwilling to make a long journey? If he will not do the ordinary cases, how is he to tackle the urgent and extraordinary. On the other hand, we are bound to admit that the highest excellence is only compatible with some specialization. The concentration of intellect on the investigation and treatment of a circumscribed section of ailments has produced the happiest possible results; yet this is no proof that all who cannot do this should shun the treatment of those ailments. It has never been held that because a man cannot amputate with the dexterity of the great masters of surgery he is therefore never to remove a limb. Let every surgeon make himself, as an individual, as competent as he can to deal with every case in which he can do good, and he will fill a useful and honourable place in the community. But speaking as I now am in the Medical Society of the largest School of Medicine in the world, I would plead for a somewhat better teaching, specially clinical teaching, of abdominal surgery. What follows refers to many schools, not to this alone. When an ovarian operation is in prospect, what I have generally found is, that a house surgeon asks for some book that he may get a knowledge of what is expected of him. I recently asked a man, What proportion of students do you think see an ovariotomy? Five per cent. was the answer. Another when questioned had seen one incomplete ovariotomy, and when he passed could not have described the steps of an ordinary operation. They were good men, both diligent students; one had been resident in the hospital of his school. Now, what would any student expect from the lovingkindness of an examiner if he could not have described a lithotomy? Yet it is much more likely that he will have to assist at or perform the former than the latter. Experience with ovariotomy is unquestionably of great value, but it only comes with time and through much error. Education goes a long way to give it minus the errors.

I will now describe the method by which most of the models before you were made:—

The cyst is first firmly packed with sawdust or bran,—this must be tightly done, otherwise the shape will not be preserved. The small secondary cysts must be opened freely from within and filled before the principal cyst is packed. It is a convenient thing to partly imbed a piece of wood in the sawdust to act as a handle to hold the cyst by while the latter parts of the preparation are being

done. A hammer answers very well. The opening in the cyst should then be pasted over and the whole allowed to dry. If it is desired to remove the cyst afterwards, it should then have a thin coating of wax or paraffin. As a rule, I have not done this; it takes a good deal of extra time and trouble. Next, the whole is carefully coated with gelatine, coat after coat, each being allowed to dry before the other is put on, until the cyst no longer dimples to the finger. I have found it very convenient to dissolve the gelatine in acetic acid somewhat diluted. This diminishes the setting property of the gelatine, and makes it more easy to work with and to get free from air bubbles, though it makes it longer in drying hard. This painting with gelatine is a slow process with a big cyst; many take a fortnight, each coating of gelatine requiring three or four days to dry. But it does not occupy much of one's time, as a few minutes suffice to give each coat. When the whole is dry and hard I take out the packing, close the opening with a sheet of gelatine, and then have it painted. With sufficient care translucency can be left; but this I have not done, owing mainly to the difficulties of successfully painting it. The casts I show have been painted for me by a local painter, Mr W. Hill. I showed him the recent cysts, and he, as well as he could, reproduced them from recollection. I would be much indebted to any member who could point out to me a ready method of preserving tissues without alteration of colour for a month. I have tried various plans, but have never succeeded. Could I put a fresh specimen and model before him together he would give me an exact reproduction.

Dr Angus Macdonald was both interested and gratified with Dr Maclaren's paper and the excellent results he had obtained. He was sure Dr Maclaren need not apologise for country surgeons, who undertook such operations in face of the difficulties they had to contend with in small hospitals. He thought they were as well provided for as some of the surgeons and others who had the special management of such cases in large hospitals. He himself had to begin by operating in a general ward, not having a private room where he could put ovariotomies. He was rather disappointed to find Dr Maclaren clinging to the use of ether in this operation. Being an Edinburgh student, he should have thought he would have had a fondness for chloroform. His experience had convinced him that it was a pleasanter and equally safe anæsthetic as the ether. He had tried ether alone, and ether mixed with chloroform. With ether alone he had repeatedly had the disagreeable result of bronchitis following an important operation. Then when the ether was given mixed with chloroform, as recommended by Erichsen, the patients got very little of the ether, as it was so much lighter than the chloroform, so that it was really an expensive mode of administering chloroform. The ACE mixture he regarded as an absurdity.

Inhalation was an awkward way of taking an alcoholic stimulant. What he preferred was to give the patient a good ounce of brandy or so before she was put on the table. The opinion that vomiting was more frequent after chloroform he believed to be a mistake. The occurrence or not of vomiting depended upon the after management of the case. If the case was progressing favourably, it was impossible to give too little food or drink the first forty-eight hours after operation. If such patient had nothing but a wet cloth to suck during that period, she would not suffer from vomiting. In case of depression it was necessary to administer an alcoholic stimulant freely, but he believed absolutely no food should be given for the time mentioned. In regard to the treatment of the pedicle, he was glad to hear that Dr Maclaren used the ligature. Notwithstanding the surpassingly good results obtained by Dr Keith and others by the hot iron, he had given the cautery up entirely. He had lately opened the abdomen twenty-seven times in succession without a single bad result, and in none of these cases had the hot iron been used, but only silk ligatures. In managing the abdominal wound there is considerable advantage in being particular about the deep stitches. They should be numerous and near the edge, consequently they could be tied more firmly. There was less tension on the individual sutures, and they could be left in longer. In regard to drainage he differed from some authorities, but he disliked it under the conditions in which Dr Maclaren and himself had been placed. Whilst he believed in the necessity of extreme caution not to admit any foul material into the abdomen, he did not drain so much as he used to do. He was anxious to have everything sealed up as soon as possible, to have everything of danger kept out. In regard to the question of antiseptics, he thought their practice in Edinburgh was different from Dr Maclaren's. Dr Maclaren was evidently thorough. The chimney was shut up, and the air saturated with carbolic vapour. But the doctor must remember that he had to get in and out by the door, and the first opening of the door admitted air which he could not disinfect. He thought further it was impossible to disinfect thoroughly by means of the spray. It was important to disinfect everything that had to go into the abdomen—hands, sponges, and instruments. His hands and arms before every operation were carefully washed, first with soap, then with turpentine, then with a corrosive sublimate solution (1 to 2000), and after that with carbolic lotion. They were thus rendered thoroughly aseptic. His sponges after an operation were washed in cold water, and put in a soda solution for twenty-four hours. They were then soaked for some little time in a 1 to 20 carbolic lotion, after which they were dried, and kept so till they were required for the next operation. They were then put into a strong carbolic lotion, and from it into water recently boiled, from which they were squeezed dry before being used in the abdomen. With

such antiseptic precautions there was no need of troubling about the spray. While believing that it did not aid much in disinfection, he did not agree with those who attributed to it poisonous effects when properly used. But he could speak of one case that was poisoned with carbolic acid, in the days before they knew how to use it, from a strong spray blowing right into the abdomen. There was now no danger of poisoning, but the spray was a bother. If it were of little use, away with it, to render manipulation more easy and rapid. Dr Maclaren, he was satisfied, need not trouble himself about the air further than to have good ventilation. He thought that as time went on the ideas on which the use of the spray was founded would become things of the past. As to the teaching of these operations, they were trying in Edinburgh to do something for students. It was an inconvenient matter to have too many breaths in the small rooms in which they had to operate, but they could easily get on with a dozen or so. He had done so in his operations, admitting students in rotation from the various clinics, and he last winter devoted three of the few clinical lectures which were at his disposal to laparotomy, hysterectomy, and the removal of the uterine appendages. A word about the removal of these latter. He was glad that Dr Maclaren had succeeded, even though the patient was still rather an invalid, in removing this tumour of the Fallopian tube. These tumours were very dangerous indeed. It was said they were not met with in post-mortems, but that was not the case. He had met with them, and they would be more frequently met with if looked for. They were slumped as peritoneal cases. They burst, and the examiner was satisfied if he found peritonitis, and did not look for pyo-salpinx. With regard to removal of the diseased ovaries, they who were convinced should, he thought, speak out on behalf of that class of women who were suffering agonies from such removable causes as dilated tubes and cirrhotic ovaries. He had satisfied himself from practical experience that these were removable causes. He was not afraid of the cry that the operation was being overdone. That objection could only be met by the conscientiousness and care of the individual operators. There were going about from doctor to doctor patients who suffered untold misery from cirrhotic or cystic ovaries or dilated tubes who could get rid of these troubles, and surely they should be told so, and have an opportunity given them of getting this done if they wished it. They formed a class of cases that had been successfully operated on by Mr Lawson Tait and many others, but who previously to Mr Lawson Tait's observations and labours were called ovarian neuroses, irritations, etc. These names would soon be things of the past. He had been surprised to find in every case of this kind in which he himself had operated that the pathological changes were much greater than the bimanual examination would have led him to expect. There was

little good to be expected from the removal of one ovary. He had failed in one case to remove both from the difficulties of the manipulation. He had not had the courage to do it. That patient was not much better; but in all the others in which both were removed the result was very satisfactory. In many of them there was a good deal of irritation for some time in the cicatrix, but this went quite away in time. If he were Dr Maclaren, he would give the patient referred to in his paper the chance of having her other ovary removed, and he believed she would be then all right. It was a curious fact that about two-thirds of those who had both ovaries removed retained sexual feeling; the remainder lost it entirely.

Dr Foulis said, that after having followed Mr Lister from Glasgow to Edinburgh, and seen the spray used with the most magnificent results, and having seen two hundred cases of ovariotomy done without the spray with only ten per cent. of deaths, he was at one time staggered. Certainly both sides could not be right. He was as firm a believer as ever he was in the potency of germs to do mischief when they got into wounds. As long as the skin remained unbroken, pathological fluids which formed within the body like ascitic or abscess fluids did no harm by blood poisoning; but let those fluids be exposed for a short while to the action of the air, or the suspended particles, living or dead, in the air, the patients were in danger of pyæmia or septicæmia. For his part, he believed that this was due to fermentation of those particles or germs which entered the fluid. Was there then any means of destroying the germs or filtering the air of them, and preventing their access to wounds? The only means hitherto employed for doing so was the spray, and he would ask whether the spray had not been of the greatest service in altering their surgical results. Dr Macdonald had strongly remarked that the spray was of no use in ovariotomy. That it was a useless bother, and interfered with the operator. He doubted whether it was advisable to abolish a method which experience showed had been of great service in ovariotomy. He questioned whether any ovariotomist had had such experience as Mr Knowsley Thornton with the spray. He was one of the youngest and most successful of operators. His percentage of deaths had lately been seven or eight, and he used the spray in the most thorough manner. He had never had a case of carbolic acid poisoning with the spray, and he (Dr Foulis) did not think that if it were used properly the spray could produce this poisoning. Dr Macdonald had operated with marked skill and success as yet, but he was struck in seeing him operate the other day by the fact that he breathed heavily right into the patient's abdomen. His breath might have carried myriads of germs in, some of them perhaps cholera germs. Why did not the germs in that abdomen come to life and give rise to blood-poisoning? He

could not tell, but he was as convinced as he lived that if a knee-joint were opened and exposed to the air not under spray, the patient was in the greatest possible danger of dying of blood-poisoning; but let it be done carefully and scientifically, as Sir Joseph Lister did it, then there was no such danger; and if the spray could be used so, well, why should it not be used? This, perhaps, was not a point intended to be raised by Dr Maclaren, with whose results he was very much pleased. It was very remarkable that there should be so many operators for ovarian disease in all parts of the country. Every one was now operating, and what would be the state of affairs ten years hence, no one knew. Every one who tackled such cases as Dr Maclaren had done deserved to be encouraged by their appreciation of his results. As to the diagnosis of ascitic fluid, that was a matter very difficult to discuss on the present occasion. There were so many methods of diagnosis described. One was struck with the number of cases in which the abdomen was opened and no tumour found but wind or water. A tympanitic tumour should be easily diagnosed, but in other instances it might be necessary to distinguish by aspiration. After ten years of observations made on fluids withdrawn by the aspirator, he found that ovarian fluids never threw down a precipitate of a fibrinous character. An ovarian fluid was always a pure cellular secretion. An ascitic fluid was always the result of obstruction to the circulation, or of inflammatory action in the peritoneum, and ascitic fluids allowed to stand for a short time nearly always showed a precipitate with the character of a felted material under the microscope. If they tapped the patient and subjected the fluid to this test, two or three days would suffice to tell in cases in which there was any doubt. The deposit in ovarian fluids showed cellular, not fibrinous, elements under the microscope. He could not conclude without referring to the models, and stating to Dr Maclaren his opinion that he had made a very great advance in preparing these cysts for preservation. He should like to ask if Dr Maclaren had preserved hearts or other pathological organs in this way.

Mr Joseph Bell said that with reference to the anæsthetization of patients, he had adopted some time ago a method which he had seen recommended the other day by a German surgeon as something new. This consisted in the administration of an ordinary hypodermic injection of morphia and atropia a little while before the operation. The amount of chloroform required was much diminished. This was done as a matter of routine. Dr Foulis had started the much-vexed question of the spray, but he seemed to settle the matter for himself when he admitted that the myriads of germs expired by Dr Macdonald into his patients' abdomens never did them any harm. Since they were on the personal line, it might be interesting to note that Eberth of

Zurich had proved the micrococcus of sweat to be a most active germ, succumbing neither through carbolic acid nor nitric acid. They all knew how the apostle of the spray was wont to perspire over a long operation, and they knew also how the sweat-drops used to fall through the spray into the open wound before him. Yet they never did any harm, although from Eberth's observations the spray could not have destroyed or enfeebled the micrococci in them. He was sure the spray was not of any benefit during ordinary surgical operations. Keith's observations should have convinced them that it was of no use in ovarian operations.

Dr Blair Cunynghame was struck by one thing, that it was not unusual even for so-called antiseptic surgeons to wear special coats in hospital. These coats were, many of them, covered with old blood-stains and discharges, yet their proximity to patients did not appear to do any harm. They also knew that menstruating women were at one time supposed to be the cause of the tainting of meat they touched.

Dr James said that some years ago he did a little work in connexion with the chemistry of various pathological fluids. He had then seen described a means of diagnosing between ovarian and ascitic fluids which was more rapid than that mentioned by Dr Foulis. This was to observe the tension of the fluids through a bent tube connected with a manometer. The tube was attached to a cannula in the abdomen, and while the tension of ascitic fluids was found to be about 6 or 7 inches, that of ovarian fluid was nearly double.

Mr Cathcart said, in reference to the preservation of the colour of specimens, that he had found carbolic lotion (1 to 200), chloral, and weak solutions of corrosive sublimate, act very well for some days. He would suggest, however, that it would be better to make a rough sketch of the tumour at once, and by means of this the artist would be able to get a better colour. He felt that Dr Maclaren had thrown a new light on the preservation of such tumours; but his method, he considered, was suitable only for thin-walled cysts.

Dr Maclaren said he used ether in every case, because vomiting was much less common after it than after chloroform. The administration of ether used to be very tedious, but since Clover's apparatus had been used, it took no longer to anæsthetize the patient than it did with chloroform. The physiological evidence was that ether was a safer anæsthetic than chloroform. The ligature was, he thought, a most admirable way of dealing with the pedicle, and one felt much more at ease when there was a ligature rather than a seared edge. The carbolized catgut ligature checked bleeding, and did not destroy the tissues. As to the spray, his own feeling was that he did not do so well in hospital without it as with it. He was in an hospital the older part of which furnished most of the material for a prize essay on pyæmia. With the spray they hardly ever saw pyæmia. He was

convinced that some germs could be admitted to wounds with impunity. Others were absolutely deadly. He had noticed that if he got a case of erysipelas in the ward, which was very rare, there were usually three or four wounds which were not being treated antiseptically. He had listened with great pleasure to a method of distinguishing between ascitic and ovarian fluids. The case in which he had opened the abdomen without having made the diagnosis was probably one of malignant disease of the peritoneum. Aspiration had been done, and the fluid examined microscopically, and only the occurrence of granular cells was noted. He had tried some of the preservatives mentioned by Dr Cathcart, with whom he agreed that the gelatine mode of preserving ovarian cysts was only applicable to thin structures which could be preserved in the dried condition.

2. A CASE OF LEFT FACIAL PARALYSIS, WITH STRABISMUS CONSIDERED CLINICALLY AND WITH REFERENCE TO LOCALIZATION OF THE LESION.

By Alex. Thom, Jun., M.D. and C.M., Crieff.

In view of recent advances in cerebro-spinal pathology, and more particularly as regards localization of a lesion, I have ventured to narrate the following case, not because of anything unusual in it, but because it presents, as it appears to me, in a striking and typical manner, those clinical features which enable us to diagnose, with a precision almost amounting to a certainty, the actual seat of disease, the pathological process, and the morbid anatomy. The case ended fatally, but I was unable to obtain a post-mortem examination. I feel, therefore, that to a Society like this an apology is due for bringing before it a narrative of clinical observations, whose chief interest centres in the verification or otherwise of the diagnosis founded upon them.

The patient was a boy nine years of age, and came under my observation on the 12th October 1883. At the time when he was taken ill, three weeks previously, I happened to be from home, and he was attended temporarily by another practitioner. When I was called to see him, his symptoms briefly were frequent vomiting, listlessness, inability to retain his head long in the erect position, staggering gait, occasional pains in the head, left facial paralysis and left internal strabismus, with some degree of paresis of the left arm and leg.

Family History.—His father is somewhat under middle height, spare, but seemingly quite healthy, intelligent, and better educated than is usual for his social station, and of a nervous temperament. He is a journeyman cabinetmaker. Two paternal uncles are alive and well. Three paternal aunts are also alive and well. One has had acute rheumatism. Another died, at the age of 31, in the

Edinburgh Infirmary, whither I had sent her. She is supposed to have had a fright, after which she had two left-sided hemiplegic attacks. During the last attack, for which I attended her, she had aphasia. As she recovered from this, epileptic fits, from which she had suffered previously, recurred and got worse. She was markedly hysterical. In the Infirmary her disease was diagnosed melancholia. Dr Byrom Bramwell has kindly furnished me with a report of the post-mortem examination made by Dr (now Professor) Hamilton, and from it I extract the following:—"Complexion, dark; eyebrows, bushy; eyelashes, long. . . . . A few atheromatous patches on ascending aorta. . . . . One or two pin-head gray nodules on the pleura. . . . . Liver slightly fatty. . . . . Vessels of dura mater somewhat anæmic. Considerable sub-arachnoid serous effusion at the vertex; 2 oz. fluid in lateral ventricles; a similar amount of sub-arachnoid effusion at base of brain; vessels of base healthy; well-marked granulations on ependyma of the lateral ventricles; vessels of optic thalami and corpora striata contained much blood, and were dilated both here and in the hemispheres. Medulla oblongata somewhat hard in the region of the corpora olivaria."

The paternal grandfather died at the age of 65, having had rheumatic fever when younger and consequent heart disease. Latterly he suffered from fits, in which he fell forwards; his breathing became almost imperceptible, and stupor followed. He is said not to have been convulsed. He died of the cardiac affection. His paternal grandmother is said to have been healthy, and died at the age of 40 of erysipelas in the head. His mother is very subject to diarrhœa, and suffers almost constantly from aphonia—otherwise healthy. Her relatives are said to have lived to an old age. The patient has three sisters, all of whom have a strumous appearance, one of them particularly so.

The surroundings at home are not the most conducive to good health. The house is small for the family, and the rooms are small. A number of birds are kept. Piggeries are rather near the back door, and the ground rises behind the house, making the back wall damp.

The patient is said to have been the healthiest of the family until two years ago, when he had measles, and has never been robust since, though never really ill. As to the present illness, the first symptom to call attention was persistent vomiting, which had come on gradually. This got rather better under treatment, but has recurred. Previous to the marked vomiting, he got a fright with a horse about seven weeks ago, and from that the parents date his illness. Some two months ago, however, his mother fancies she noticed something of a squint in the left eye. She says also there was often a twitching of the left arm when it was hanging loose, but cannot say she has noticed anything like that in the leg. After the vomiting had attracted their attention,

the parents say they noticed the squint, that he did not speak so well, and that he was difficult to please and often listless. Also, that his expression was peculiar, that his left arm and hand were powerless, and the left leg weak.

I visited the patient first on the evening of the 12th October, and found him sitting on a couch resting his head on his right hand. He says his head feels heavy, and is easiest when supported. A glance tells that he is far from well. He has marked convergent strabismus of the left eye, paralysis of the facial muscles of the left side, and the peculiar expression resulting therefrom. The mouth is drawn to the right side, all natural folds being gone from the left. The tongue when protruded (an action which is performed easily, and without any jerking or tremulousness of the organ) is pointed to the left, the paralyzed side. On the left side of the face the only muscular movement which can be performed at will is half closure of the eyelids. The left arm and leg are weak, but not paralyzed; the muscles feel softer than on the right. He can use the arm, hand, and fingers, but with an effort. When sitting he can move the leg with comparative freedom.

He is an intelligent boy, and can read nicely. Though strabismus is present and some degree of diplopia, vision is good in the eye, as he read very well to me from a lesson-book, keeping the right eye closed. Speech is impaired, but not more so than would be expected from the facial paralysis. Memory and power of attention remain unimpaired. As far as muscular power is concerned, he can walk; but he is much inclined to totter and reel over, particularly to the right side. I have satisfied myself that this reeling is not due to any giddiness; nor yet is it due to the strabismus, the same symptom being present in the same degree whether both eyes are open or only one (and it matters not which one). It is evidently due to deficient power of co-ordination.

The left eyeball does not remain steady. It presents that condition to which the term "nystagmus" is applied. I examined the eye ophthalmoscopically with some difficulty, in consequence of the constant movement, and thought there was some hyperæmia of the left fundus, but could not make out more. The pupils both contract to light, and in accommodation for near objects.

There is some diminution of the sense of hearing on the left side,[1] otherwise the special senses and general sensibility are normal. The palate on the left side is pendulous, and the uvula points somewhat to the right. There is sometimes a little difficulty in swallowing. The appetite is bad, and the stomach easily turned. The phenomenon of patellar tendon reflex is present on the left side, though not so marked as on the right. He has never had any convulsions.

[1] Below the lobe of the left ear, in the region of the parotid gland, there is a spot tender to pressure.

Temperature normal. Pulse is about 100. The heart normal. No cough. Bowels a little loose. Urine passed normally. Sleeps well and soundly. Urine contains a trace of sugar; no albumen or bile; sp. gr., 1018; reaction, acid. No increase of phosphates.

Provisional Diagnosis.—The internal strabismus denoting paralysis of the abducent or external rectus muscle of the eyeball, supplied by the sixth cranial nerve, the paralysis of the facial muscles supplied by the portio dura of the seventh, and the diminution of the sense of hearing on the left side, point to a central lesion involving these nerves, probably in the medulla oblongata, where their nuclei are situated. The paresis of the left arm and leg, taken in connexion with the other symptoms, indicates some similar affection of more central nervous mechanism connected with these extremities, *i.e.*, in the brain or spinal cord, rather than in the nerve trunks.

Prognosis.—My first idea was that the lesion might be of the nature of a Polio-myelitis, and therefore I thought that the prognosis might be hopeful as regards life, though by no means so as regards recovery of the paralyzed facial muscles. Further examination led me to change this opinion.

Treatment.—A small fly-blister behind the left ear and above the tender spot mentioned; 1 grain of iodide of potassium four times daily; good diet, mostly milk; quietness, freedom from excitement, and as much fresh air as possible.

Further Examination Next Day.—The muscles of the right arm and leg respond well to the faradic current, the left not so well. The facial muscles do not respond well on the left side. There is some contraction near the chin and at the side of the nose, and a little on the forehead. The orbicularis palpebrarum acts a little, but the other muscles do not move to a medium interrupted current.

With the constant current the muscles, both of the face and the extremities, show very nearly a normal reaction on both sides; but contraction is somewhat more easily obtained on the left side. There is no reaction of degeneration.

The knee jerk is impaired on the left side. By careful ophthalmoscopic examination, I was able to make out that in the left eye the optic disc was less defined, seemed larger, and was of a redder colour than in the right eye. In fact, that condition was present known as optic neuritis. The right disc was also less defined in outline than it normally should be, but to a less extent than the other.

This condition of the eyes, taken in conjunction with the electrical condition of the muscle, led me to make the diagnosis of intracranial tumour in the situation already indicated. As I thought its nature was probably tubercular, I considered the prognosis bad.

To the former treatment I added the daily use of a mild faradic current to the paralyzed muscles.

The iodide was continued for about eighteen days, and then stopped in consequence of a bad taste in the mouth. In some respects there has been improvement. There is less vomiting and less tendency to reel when walking.

By the end of October the facial muscles responded well to the faradic current, most distinctly so to the secondary one. They could be made to contract both by direct stimulation and by stimulation of the motor points. The eyelids can be closed under electrical stimulation. The constant current could not be said to give any information; and as the patient seemed not so well after its application, that form of electricity, which had not been used since the first examination, was not had recourse to again. The muscles of the left arm and leg responded to the faradic current in a normal manner, and the patellar tendon reflex equalled the right.

About the end of October he began to complain of occasional pain in the left upper frontal region. This increased, and on 2nd November he was ordered 6 minims liq. ext. ergot and 5 minims tr. bellad. three times daily. A little bromide of potassium was occasionally given at bedtime. After five days and no improvement, the belladonna was stopped and the ergot continued. In four days more this also was discontinued, vomiting being troublesome, though evidently partly of gastric origin this time. The tongue was furred and dry.

9th Nov.—Left pupil smaller than right. Left eyeball much congested. Pain in frontal and parietal region severe. Vomiting troublesome. Tongue clean. Discontinued all electrical treatment.

On the 12th Nov. it is noted that the left eyeball was very much congested superficially, and that an ulcer had appeared on the cornea. This was treated by solution of nitrate of silver, and by strapping the eyelids together. He was ordered 5 grains iod. pot. four times daily. Under this treatment some temporary improvement took place.

19th Nov.—Seems rather better. Can hold up his head a little. No pain. Sleeps well. Very little vomiting. The paralyzed facial muscles do not respond so well to the faradic current, which was tried to-day for the first time since he was worse. He cannot raise the right arm at the shoulder, the biceps and deltoid muscles being inert, but contract on faradic stimulation. Continue the iodide of potassium.

24th Nov.—He has improved considerably in general health. Very little vomiting; a fair appetite, but requires aperients frequently. To-day he is not so well. The right arm and hand are powerless; the right leg can be moved with difficulty, and knee jerk is exaggerated. The paralyzed facial muscles can only be moved by strong faradic currents, so strong as to cause pain. Unsatisfactory contractions are produced on the right side also. The left eye is becoming disorganized. The right presents the

condition of optic neuritis. Pulse only 80, though he has been having five grs. iodide four times daily.

From this date until the 30th, when he died, he sank rapidly. Total paralysis of the right extremities came on with exaggerated knee jerk, and diminished reaction to electrical stimuli. On the left side also the knee jerk became somewhat exaggerated. Sensibility became impaired on the left side of the face. The left eye was totally disorganized. The right facial muscles retained their power to the last. The pulse gradually got slower, and the temperature was sub-normal. Swallowing and speech became difficult, and frequently the respiration was laboured. Consciousness remained until a few hours before his death. Urine was voided naturally. For some hours before he died the right arm and leg were jerked with considerable force. Death ensued upon cessation of the respiration, the pulse beating with considerable force to the end. Convulsions were never present.

Gentlemen,—I regret to say my record of the case is ended. To my great disappointment a post-mortem examination was not permitted.

Remarks.

First, as to Diagnosis, and in this connexion we must keep in mind the family history, which can only be characterized as bad. On the father's side it is of a markedly neurotic type, and in the case of the paternal aunt we have proof of the existence of nervous disease. Considering the mother's health and the constitution of the sisters, we have those conditions under which we are not surprised to find tubercular disease ensuing. But more particularly, the paralysis of the facial muscles of expression indicates some affection of the portio dura of the seventh cranial nerve, and the question arises, Is that affection peripheral or central? That the lesion is above the geniculate ganglion is proved by the paralysis of the soft palate and the distortion of the azygos uvulæ. Further, electrical examination showed that the lesion was not situated in the nerve trunk, there being no reaction of degeneration present, and no atrophy of the affected muscles, and faradic irritability remaining, though in a diminished quantity. Besides, we have symptoms which point directly to a central lesion. The staggering gait was independent of the strabismus, and was such as is found to occur from lesions of the cerebellum or its peduncles. (Since studying this case I have had the pleasure of reading Professor Grainger Stewart's *Lectures on Giddiness,* and can assure him I had satisfied myself that here we had the tendency to stagger without the subjective feeling of giddiness. He points out that this is possible.) The paralysis of the facial muscles was complete; the velum palati was also paralyzed, and, in addition, the externus rectus muscle of the eyeball was paralyzed. Also there were latterly paralysis and

paresis of some of the muscles of the extremities, difficulty of swallowing, and disorder of speech.

These facts, taken along with the progressive character of the case, the presence of double optic neuritis, the headache, vomiting, and impairment of hearing, point conclusively to the central nature of the lesion.

As to Localization of the Lesion.—As far as the facial paralysis is concerned, the lesion might be situated anywhere between the motor centres on the surface of the cerebrum and the point of origin of the seventh nerve from the medulla. Similarly as regards the paralysis of the extremities, the lesion might lie anywhere between the motor centres and the segment in the cord above that from which the nerves to these extremities arise. It certainly did not involve the spinal origin of the nerves, because the existence of patellar tendon reflex proved that the spinal arc was intact. The cerebellar reel indicated lesion affecting the cerebellum or some of its peduncles. Now, we can lay our finger on a part where these various tracts are in close juxtaposition. There is little doubt as the nuclei of the portio dura of the seventh nerve and of the sixth nerve lie close together in the medulla oblongata, and as both these nerves were paralyzed, that the lesion was in the neighbourhood of these nuclei and involved them. The cerebellar reel is easily accounted for by supposing that the lesion implicated the middle peduncles of the cerebellum which pass through the pons varolii, and are in the neighbourhood of the sixth and seventh nuclei. And further, the paralysis and paresis of the extremities are explained by implication of the pyramidal fibres of the medulla, by which voluntary motor impulses are conveyed from the cortex of the brain by the corona radiata, the anterior two-thirds of the posterior division of the internal capsule and crus cerebri to the crossed and direct pyramidal tracts of the cord, and thence by the anterior nerve roots to the muscles. Taken along with the other symptoms, the glycosuria, though slight, may be mentioned as pointing to the region of the fourth ventricle.

As to the Nature of the Lesion.—That it was not a myelitis is proved by the electrical irritability of the muscles and the presence and exaggeration of the phenomena of patellar tendon reflex; also there was no reaction of degeneration, and no increase of temperature, though the disease was progressive. Further, I find that Dr Buzzard, in his *Clinical Lectures on Diseases of the Nervous System*, points out that a myelitis, which could produce such paralysis of the sixth and seventh nerves and also of the extremities, could hardly exist without involving those centres which are necessary to the circulatory and respiratory functions, viz., the central nuclei of the vagus. The lesion which, I think, the observed phenomena and the course and sequence of the symptoms point to, is tumour of the medulla oblongata. At all events, if we suppose the existence of such a tumour we can offer a rational

explanation of the symptoms during life. If we assume that only one tumour existed, there are some features in the case which require a little consideration, and therefore I shall dwell on this point a little more in detail. Before doing so, however, I would merely remark that the condition observed in the fundus of the eyes, viz., optic neuritis, points almost, if not quite conclusively to intracranial tumour. It is quite well known that a tumour of the pons, on a level with the points of origin of the sixth and seventh nerves, can produce symptoms such as those enumerated. Suppose a tumour originated in the formatio reticularis on the left side at this level, it would first produce paralysis of the abducent nerve from pressure on its nucleus, as it grew forwards would press on the transverse fibres of the pons and cause reeling. Enlarging to the left it would press on the seventh nucleus and produce facial paralysis. As it grew forwards still further, it would compress the fibres of the left pyramidal tract, and cause crossed paralysis of the right extremities. The further course of the disease is also easily explained, for as the tumour grew downwards it would compress the nuclei of the ninth, tenth, twelfth, and also the eleventh nerves, giving rise to the more purely bulbar phenomena and death. "As it grew upwards it would compress the motor and to some extent the sensory divisions of the fifth nerve, causing the masticatory paralysis, the facial sensory disturbances, and the neuroparalytic ophthalmia" (Ross). Now, in the case before us all this is quite clear. The first symptoms observed were headache and internal strabismus. Then came facial paralysis and staggering gait. Afterwards (omitting in the meantime the left-sided hemiplegia) there followed at an interval the crossed paralysis or right-sided hemiplegia. The other symptoms followed in sequence, the muscular jerking observed before death being doubtless due to a local meningitis set up by pressure.

One point is not quite clear, viz., What was the cause of the early paralysis and afterwards paresis of the left arm and leg? Paresis, be it noted, on the same side as the facial affection and the assumed central lesion. Note, too, that this paresis was accompanied by diminution of the phenomena of patellar tendon reflex on that side. This led me to think at first that there might be a polio-myelitis anterior acuta vel chronica. The electrical reactions of the muscles showed me that this was not the case. We have already seen that on the right side, when paresis of the extremities manifested itself, it was accompanied by exaggeration of the knee jerk. This is just what we would expect from lesion of the left pyramidal fibres, by which inhibition of that reflex phenomenon is conducted to the cord. In the case of the left hemiplegia there might have been some irritation of the right pyramidal fibres, and so exaggerated inhibition and diminished reflex action. But this seems unlikely. One explanation of the early left hemiplegia with diminished knee jerk is that the tumour may have originated

near the middle line and grown irregularly; that, at first growing rather forwards and to the right, it pressed on the pyramidal fibres in the right side of the medulla, but that, finding less impediment to its growth in other directions, its further increase was in the lines already indicated. We may then assume that the pyramidal fibres accommodated themselves to the slight pressure and recovered their functions, the paresis of the muscles and diminution of electrical reaction and of the knee jerk depending upon disuse, and all being recovered from under electrical treatment.

As regards the Nature of the Tumour.—Two forms are common in children—tubercular tumours and gliomata. The family history and the presence of the enlarged cervical gland point to the tubercular as the more likely form. Dr Ross records two cases of tubercular tumour, but not occupying quite the position which I have indicated.

Lastly, as to Treatment.—Once a tumour has begun, I am afraid that at present we have no means at our disposal whereby we may expect to stay its progress. Our treatment must consist in attending to the general health and combating symptoms as they arise. I am not without hope, however, that sometimes, when the tumour or tumours are small and purely tubercular, the internal administration of iodine in some combination, possibly as iodoform, may prove efficient. Though, from what I have read lately, my doses seem ridiculously small, I always thought that this patient was brighter and more comfortable after he resumed taking five grains of iodide of potassium four times daily. (On this, however, I am not inclined to lay very much stress, for I have noticed in several cases of progressive disease of the nervous system, that the tendency of such affections is to advance by exacerbations. After each exacerbation, which causes a general shock, there is gradual improvement, but the patient never attains quite to the condition in which he was before the relapse ere another exacerbation occurs.)

The important point for us to remember is, that prophylaxis may accomplish much. It is of the utmost importance that members of a family with a neurotic and strumous history such as this be kept in conditions least likely to prove hurtful. To pursue this further would add unnecessarily to the length of my paper, and be departing from the intention of it. Moreover, I have already trespassed too much upon your time and patience, for which I beg to apologise.

The President was much disappointed that there had been no post-mortem examination. The want of it rendered the case hypothetical, and took greatly from its interest.

Dr Alex. Bruce had agreed with almost every word Dr Thom had said as to the diagnosis. He thought he understood him to

say that the case began with vomiting, and was followed by left internal strabismus, left facial paralysis, then weakness of the left side, followed by paralysis of the right arm and leg, and at an early period of the case there were some impairment of hearing and a certain amount of reeling, not due to the strabismus nor to the weakness of the limb, evidently, therefore, due to some cerebellar interference. He did not see, unless there was more than one lesion present, how the symptoms could be explained in any other way than that described by Dr Thom. The increase of galvanic reaction and diminution of the faradic, he should be inclined to understand as a partial and not complete reaction of degeneration. There were many cases in which the faradic reaction was absolutely absent and the galvanic reaction markedly increased, and where the contraction was markedly slow. The condition was due to this fact, that some of the nerve-fibres were left unimpaired, and consequently some of the muscular, so that they had not a complete absence of the faradic reaction, and not a complete development of the galvanic reaction. This might account for what Dr Thom described, and where the destruction of the nuclei of the centre was sufficient to cause almost complete paralysis of the muscle so far as its voluntary functions were concerned, and yet not to give a complete reaction of degeneration. He thought Dr Thom's view as to the seat of the lesion was borne out by the fact of the palate being involved, as also the sense of hearing, and an incomplete involvement of the centre was borne out by the reaction of degeneration not being complete—the lesion growing first downwards and forwards into the pyramidal tracts of the right side, and pressing these aside, and possibly causing some interference with their conducting power, and then relief being given, and by the tumour growing backwards to the right side, pressing on the right pyramidal tract before it had crossed, and causing the right-sided paralysis. It was much to be regretted that a post-mortem examination was not obtained. It was seldom that a case was described so accurately, and it would have been of great value to have had confirmatory evidence of the diagnosis.

Dr Foulis wished to mention a case that had come under his observation lately. In January 1884 a lady consulted him for pain in the left occipital region. A week after she fell into a deep sleep, out of which she awoke with all motor power abolished on the right side. She recovered, went to Moffat, enjoyed the summer, came home, and fell asleep for three days. After the administration of iodide of potassium, she remained well for some weeks. Six weeks before death she fell into a deep sleep. Two well-known neuro-pathologists were consulted. One of these gentlemen diagnosed a tumour of the base of the brain in the region of the sella turcica, the other a tumour of the pons variolii. The lady recovered, and three weeks before death had no symptom of any kind; but three

days before her death fell into a sleep and passed away. A post-mortem examination showed the brain healthy in all respects, except that in the substance of the left lobe of the brain, anterior to the left lateral ventricle, there was a cavity which would contain a duck's eggful of thin sanious fluid. The whole arch of the brain covering this tumour, and to the right side of the left frontal convolution, was a gliomatous tumour, and a portion of the right frontal lobe also formed a tumour. There was nothing at all in the regions where tumour was diagnosed. The symptoms he believed to be caused by the variations in pressure of the fluid in the cavity. There was nothing in the history except that ten years before she had an epileptic fit on the death of a niece. He mentioned the case to show how necessary a post-mortem examination was for the purpose of verification.

Dr Thom regretted that he had been quite unable to obtain a post-mortem examination. In reference to Dr Bruce's remarks, he had not called the symptom the reaction of degeneration, because he could not say that the contraction from the constant current was slowed, and also because it was recovered from under the faradic current after a time. Dr Foulis's case was another instance of what was well known, that serious gross lesions might occur in the brain without giving evidence of their presence.

Meeting VIII.—May 6, 1885.

Dr David Wilson, *Vice-President, in the Chair.*

I. Election of New Member.

James Robertson Crease, F.R.C.S. Ed., South Shields, was elected an Ordinary Member of the Society.

II. Lantern Microscope Demonstration.

Dr Foulis demonstrated, by means of the oxy-hydrogen microscopic lantern, the circulation in the web of the frog's foot, and also several botanical sections.

III. Exhibition of Pathological Specimens.

1. *Dr Chas. W. MacGillivray* showed the CONDYLES OF THE HUMERUS of a girl, aged 19, who about six months before had injured her elbow by a fall, dislocating the bones of the forearm backwards, and fracturing the external condyle of the humerus. She went to a bone-setter, who did not recognise the injury, and recommended that

fomentations should be applied. When brought to the Infirmary the dislocation was evident, and the elbow was fixed at an angle of about sixty-five degrees. The operation devised by Dr Watson for such cases was performed, but modified in so far that in addition to the incision between the olecranon and internal condyle another was made over the external condyle. This was done because of the difficulty of dissecting out the condyle through the single incision recommended by Dr Watson. No muscles were divided, and the operation was done sub-periosteally. The result was perfect. She could flex and extend, pronate and supinate perfectly. He had intended to have shown her to the Society, but the plasters used for making an extension had set up a rather severe eczema of the arm, which prevented him from bringing her out.

2. *Mr A. G. Miller* showed (*a.*) an EPITHELIOMA removed along with a portion of the lower jaw from a man, aged 50, who had suffered from the disease for four years. The growth was distinctly papillated,—as warty as any warts ever removed from the glans penis. The slow growth of the tumour showed that it was but slightly malignant. It had been frequently touched with strong acids, and the two teeth removed with it were quite black. The operation had been done ten days ago, and the patient was doing well. (*b.*) The HEADS OF TWO FEMORA removed from young children for hip disease. The larger of them was from a girl, aged 9. The operation was done six weeks previously. The wound, excepting the track of the drainage-tube, was healed, and she moved her limb in bed with perfect freedom. The smaller was taken from a child, aged 5, who had also made a rapid recovery. (*c.*) A large LOOSE CARTILAGE removed from the knee-joint about a month before. The operation was done antiseptically and under spray. The joint was drained by a tube passed between the external lateral ligament and condyle, and brought out posteriorly. When removed, it was found to have been quite flattened between these structures, so that it could not have been of much use. The wound healed rapidly, and the joint movement was perfect. Had he to do the operation over again he would use a smaller drainage-tube.

3. *Dr Allan Jamieson* showed a drawing of an affection which he believed to be unique—ICHTHYOSIS PALMARIS AND PLANTARIS,—the disease being confined to the palms and soles. The patient from whom the drawing was made was a man, aged about 35. His father had suffered from the same complaint, and also a sister. It began, as it usually did, in infancy. The under surface, as shown in the drawing, of the sole was occupied by a dense horny growth about a quarter of an inch thick. The under surface of the toes was also affected. On the palms the disease had not extended to the same degree as on the feet, because from the greater mobility and greater attention it was not allowed to grow to the same

extent. It was limited to the palms. The growth, as shown, had existed for six months, and had begun to separate in portions. When separated, it left underneath a dry, reddish-brown surface, hard, but not so hard as the growth itself. The rest of the skin of the body was pliable, soft, and healthy; but the nails grew with extreme rapidity at the rate of a quarter of an inch a fortnight.

IV. Original Communication.

A CONTRIBUTION TO THE STUDY OF LARYNGEAL PARALYSES.

By P. M'Bride, M.D., F.R.C.P. Ed., F.R.S.E., Surgeon to the Ear and Throat Department, Royal Infirmary; and Lecturer on Diseases of the Ear and Throat, Surgeons' Hall.

It is not my intention in this paper to present an exhaustive monograph on laryngeal paralysis, nor indeed to refer to all the various varieties which may occur. I shall attempt rather to draw attention to cases of clinical interest which I have recently met with, using them either to illustrate important points, or when they seem of sufficient importance, discussing them more in detail, and deducing conclusions.

Unilateral Recurrent Paralysis.

Perhaps the most common cause of this condition is aneurism of the aorta, which is, of course, more apt to affect the inferior laryngeal nerve of the left side. The left recurrent is also, from its anatomical relations, more likely to be pressed upon by enlarged bronchial glands. The right, on the other hand, may be, and not uncommonly is, involved in affections of the corresponding apex of the lung, less commonly it is pressed upon by aneurism of the innominate or subclavian. Other causes which affect both nerves with equal frequency are cancer of the œsophagus, goitre, mediastinal tumour, etc. In some cases rheumatic influences are said to produce paralysis, but it seems to me that, in view of the impossibility of excluding positively such causes as a very small aortic aneurism or an enlarged bronchial gland, a diagnosis of rheumatic paralysis could only be verified on the dissection table.

Having alluded briefly to the causes which may lead to paralysis of the larynx from pressure upon one or other recurrent nerve, we must now consider the signs and symptoms so produced. Until quite recently it was assumed that pressure upon the recurrent nerve must of necessity produce an equal effect upon the power of all the muscles supplied by it. It was therefore believed that the vocal cord of the corresponding side always remained stationary in that position described by Ziemssen as the "cadaveric," *i.e.*, midway between extreme abduction and adduction. Moreover,

as all the muscles were supposed to be paralyzed, it was assumed that the cord of the affected side lost its tension and became excavated at the margins, while on attempted phonation a compensatory movement of the opposite cord with crossing of the arytenoids and obliquity of the glottis occurred. The condition thus described is that which is as a matter of fact seen in cases of complete recurrent palsy, and is not unfrequently met with, causing marked disturbance of voice.

Inasmuch as the causes of recurrent paralysis are usually such as tend to increase, not suddenly but gradually, it follows that before complete paralysis results a paretic condition must subsist for a longer or shorter time. Although casual references had been made to the matter before, yet it is to Semon that we are indebted for first pointing out that the abductors of the cords are first and often alone affected by pressure upon the trunks of the inferior laryngeal nerves. Even in cases of laryngeal paralysis due to central causes, Semon [1] has demonstrated beyond a doubt that the abductors are more apt to be paralyzed than the adductors, and he seems inclined to dispute altogether the occurrence of isolated paralysis of the adductors as a result of organic change. Now, this fact is not only of purely scientific interest, but of immense importance to the physician. In those cases in which the abductor of the affected cord only is paralyzed the cord remains in the middle line during inspiration, and this is in some cases the only sign or symptom of disease. In these cases the voice for all ordinary purposes may be perfect, there is no dyspnœa, because on inspiration the healthy cord leaves ample room for the passage of air, and on phonation the laryngeal image seems perfectly normal.

From my own limited experience I would incline to believe that this position of the cord in the middle line is more common than the cadaveric in cases of pressure on the recurrent trunk. Since the beginning of the year I have examined probably not more than half a dozen cases of simple unilateral paralysis of the larynx, and of these three were free from all symptoms pointing to so grave a condition. The first case was that of a clergyman, who certainly complained of his voice being easily fatigued, but was yet able to perform his duties. Moreover, this symptom was much less marked after a coincident chronic laryngitis had been relieved, and would not, I am sure, have attracted attention had it not been for the patient's profession. His voice seemed perfectly natural, there was no dyspnœa, and yet the left cord remained in the position of phonation even on deep inspiration. The cause of the paralysis in this case remains doubtful, but it was uninfluenced by electricity, although no physical signs of disease could be

[1] "On the Proclivity of the Abductor Fibres of the Recurrent Laryngeal Nerve, etc." (*Archives of Laryngology*, No. 3, 1881). "Ueber die Lähmung der einzelnen Fasergattungen des nervus laryngeus inferior" (*Berlin Klin. Wochenschrift*, 1883, No. 46).

detected on examining the chest. Another example was the case of a gardener who applied for advice at the Infirmary on account of some vague sense of discomfort in the throat. His voice was perfectly good, but the left cord was found to be immovable in the middle line. On examination of his chest there was found a suspicious area of dulness, which, when taken in conjunction with the laryngeal condition, rendered the presence of thoracic aneurism (or other tumour) extremely probable. My third case was that of a patient who had a glandular tumour in the right cervical region, and who, indeed, consulted me for an aural affection. The voice and respiration were absolutely perfect so far as I could judge, and, indeed, there were no throat symptoms, yet laryngoscopic examination revealed the right cord immovable in the middle line.

I think cases like these show how important laryngoscopic examination may be for purposes of diagnosis, and, above all, in examining for life insurance, and I can only cordially support Semon,[1] when he writes, "Laryngoscopic examination is a duty in all organic diseases (proved or suspected) which may affect the centres or trunks of the motor nerves of the larynx, even when no laryngeal symptoms appear to demand such an examination."

I do not propose here to enter into the question of why the abductor filaments of the recurrent nerve should be so much more prone to paresis than the adductor. No satisfactory explanation seems to have been hitherto advanced. It is, however, worthy of notice that Exner,[2] in his recent paper on the innervation of the larynx, states that the lateral crico-arytenoid muscle or adductor receives its supply from both the inferior and superior laryngeal nerves, and sometimes from a nerve of the opposite side, while the abductor (posterior crico-arytenoid muscle) also receives its nerve supply from both nerves, but that derived from the superior laryngeal is very insignificant.

Bilateral Paralysis of the Abductors.

It would be quite superfluous here to enter into an account of the various causes which have been known to produce this very rare condition. Suffice it to say, that it may be due to changes in the nerve centres, the recurrent nerves, or laryngeal muscles (myopathic). I may remark, however, that the fact of the abductor filaments of the inferior laryngeal nerve being so much more prone to suffer from central and peripheral causes justifies us in considering under this heading certain cases of pressure upon both recurrent nerves. The most prominent symptom of complete bilateral paralysis of the abductors is dyspnœa, chiefly of an inspiratory character, much increased on exertion. The voice is of course not much affected, while laryngoscopic examination shows the cords to

[1] *Op. cit.*

[2] Review by Chiari, *Internat. Centralblatt für Laryngologie, etc.*, Feb. 1885.

be close together, and deep inspiration brings them still nearer to each other.

The first example of this affection which I propose to relate is the case of a hospital patient suffering from locomotor ataxia, sent for laryngoscopic examination by Prof. Fraser, to whose kindness I am indebted for further details, as also for permission to publish the case.

G. P. has within the last two years suffered from symptoms of locomotor ataxia (girdle sensation, lightning pains, and inco-ordination). There is occasionally a sensation of choking, and the patient is sometimes wakened by his noisy breathing. During sleep inspiration is high pitched and stridulous, while expiration is low and moaning; during sleep, too, the breathing at times presents a marked Cheyne-Stoke's character. Sensibility is much delayed in the left leg, and the Argyll-Robertson eye phenomena are present. Hearing is defective in both ears, but examination shows this to be due to the remains of old standing chronic suppurative middle ear disease (thickened and cicatrized membranes), while the auditory nerves are healthy. Smell and taste are normal. Patellar tendon reflex is absent. There are occasional attacks of vomiting. The tongue is put out quite straight, but on phonation the palate is slightly drawn towards the right. Food sometimes sticks for a short time in the pharynx, and there is a sense of constriction about the throat and difficulty of breathing. There is an occasional cough, and after choking attacks, which sometimes occur, the patient brings up watery, clear sputum. The heart-beat is abnormally rapid, varying from 100 to 120 per minute, while respiration is slow during sleep but quickened when he is awake. The head when thrown back can be bent more easily and fully to the left than to the right. The voice is perhaps a little roughened. The results of laryngoscopic examination were as follows:—The colour of parts normal, except that the cords have a slightly mottled appearance. The interval between the vocal bands is always small, and is slightly increased by forced expiration, while deep inspiration brings them together and causes dyspnœa. On phonation the cords come together, but there is a distinct bend in the usually straight glottic line—not unlike that described by Mackenzie in paralysis of the cricothyroid muscle. I may, however, mention that external palpation during phonation afforded no evidence of such paralysis. The larynx and pharynx were deficient in sensibility, but by no means anæsthetic, and considering the normal variations in sensibility met with, I did not feel entitled to conclude that the superior laryngeal nerves were implicated.

In this case the paralysis of both abductors of the larynx was evidently due to a patchy sclerosis of the medulla secondary to the spinal sclerosis. That the pneumogastric nucleus was to a great extent involved is shown by such symptoms as the increased

rapidity of the heart-beat, the Cheyne-Stoke's respiration, and perhaps the altered respiration, and attacks of vomiting. It must, however, be remembered that the now generally accepted view as to the motor laryngeal nerves is that they are derived from the spinal accessory, some of the fibres of which, joining the pneumogastric, are again reflected upwards in the recurrent trunks. In the case before us we have evidence of the implication of the nucleus of the spinal accessory in (1), greater difficulty in bending the head to the right; (2), slight deviation of the palate to the right; (3), laryngeal paralysis; (4), difficult deglutition. Of course every laryngologist will readily admit that the laryngeal paralysis must have been due to this cause, but I believe that it also accounts for the peculiar bend in the glottis on phonation. It will be remembered that there was a difficulty in flexing the head to the right, showing that the right sterno-mastoid was weakened, while, on the other hand, the palate was drawn a little to the right on phonation. This last named fact proves weakness of the palate muscles of the left side, and these are in part supplied by the spinal accessory nerve; but to this anatomical point reference will be made later. Then, again, the difficulty in getting food to pass the upper part of the pharynx seemed to show a weakness of the superior constrictor. I have said enough, however, to prove that the two spinal accessory nerves were in this case unequally affected, some fibres of the left being stronger than those of the right (*e.g.*, those supplying the sterno-mastoids), while again the palate branches of the right were better preserved than those of the left. I believe that the peculiar bend in the glottis on phonation was due to a similar cause, and that the vocal process of the side on which the laryngeal adductor fibres of the spinal accessory were best preserved pushed over the vocal process of the opposite side, thus giving rise to the wavy outline.

Cases of bilateral paralysis are rare, and as yet only a very limited number have been recorded in which the affection was due to locomotor ataxia. Semon has met with several instances, however, and is inclined to believe that the laryngeal symptoms of locomotor ataxia may be found due to this cause more commonly than is now suspected. The reported cases of bilateral paralysis of the laryngeal abductors due to locomotor ataxia accessible to me are as follows:—Semon (*Clinical Society's Transactions*, vol. xi. p. 141); Dreschfeld (*Medical Times and Gazette*, 1881, vol. ii. p. 362); Krishaber (*Gazette Hebdomadaire*, 1880, p. 662). Dr Semon, however, informs me that cases have been also observed by Cherchousky, Remak, Oppenheim, and Jacob, while he himself has at present two additional instances under observation.

I next come to a class of cases in which the abductor muscles of both sides are affected, but not to an equal extent; and perhaps it will best serve the purpose of this paper to record shortly an illustrative case.

Mr C. was brought to me for consultation by Dr Johnston, who, while kindly permitting me to refer to the case, intends to publish a full account of it. The voice was rough, but not aphonic; there was laryngeal stridor on deep inspiration, and some shortness of breath on exertion; while the physical signs pointed to an intra-thoracic tumour, the dulness being most marked on the right side. Laryngoscopic examination gave the following results:—Right cord immovable in the middle line; left cord abduction imperfect, *i.e.*, opening of glottis on full inspiration is much smaller than between middle line and cord on full inspiration in health. Adduction is fairly perfect, and there is a tendency to crossing of the arytenoids. The diagnosis of complete paralysis of the right abductor, and paresis of the left, was therefore justified. The patient died, and the autopsy revealed mediastinal sarcoma affecting principally the right side, but to some extent the left.

Within the last few days I have examined a case in which the condition was precisely the same, only that the left abductor was completely paralyzed and the right only partially, the cause being probably a large aneurism (possibly a tumour).

Cases like these go far to prove how exact diagnosis may be facilitated by the use of the laryngoscope in thoracic cases. They further lead to the interesting question, *Whether the routine use of this method in all such cases might not lead to the recognition of a stage of recurrent paralysis prior to complete loss of abduction in unilateral pressure upon the recurrent?* I should feel strongly inclined to attach very great importance to laryngoscopic examination during deep inspiration in all cases of suspected thoracic disease in which pressure on one or other recurrent nerve might occur; and *if in such a case one cord did not move so far outwards as the other, this fact would seem to afford as strong evidence of pressure upon the corresponding inferior laryngeal nerve as either complete immobility in the middle line or the cadaveric position.*

Paralysis of both Laryngeal (sup. and inf.) and other Nerves of one Side.

For permission to examine and describe the laryngeal and throat symptoms of the following very rare and interesting case, I am indebted to the kindness of Professor Chiene.

A. M., æt. 65, was sent to my clinic for laryngoscopic examination. There is a tumour in the left parotid region, which prevents the mouth being widely opened; but fortunately the absence of incisor teeth makes laryngoscopic examination possible, although the sinus pyriformes could be but dimly illumined. The first point to attract attention was the presence of an abnormal quantity of mucus on the left side, but its exact source could not be made out. On phonation nothing abnormal is observed, both cords being on the same level; on inspiration, however, the left cord maintains its position in the middle line. Examination with a

probe shows that the parts on the left side can be touched without producing either sensation or reaction—*e.g.*, epiglottis, region of the arytenoids, and pyriform sinus. A similar proceeding attempted on the other side at once causes cough, vomiting, and spasm. There was nothing abnormal noticed about the epiglottis, except that when its left margin was hooked upon the probe it was noticeably yielding and flabby—*i.e.*, compared with the resistance offered in a healthy larynx. There is very slight difficulty in swallowing, but it does not cause discomfort. The left faucial pillar is quite anæsthetic, and touching it produces no reaction. When the tongue is protruded, it is pushed a little to the left. Taste is absent posteriorly over the left side of this organ (the substances used being sweet, salt, and bitter), but is preserved anteriorly. The appetite is moderate. The pulse is not much quickened, and the eyes normal. There is neuralgia of the left ear and side of the head, and the patient states that he feels an impression passing from the left ear to the throat. There is no congestion of the side of the head. In the right ear there is evidence of chronic middle ear catarrh; while on the left side the handle of the malleus is congested, and the yellow discoloration of the membrane at its posterior inferior part, together with a distinctly visible line of demarcation, renders the presence of serous exudation within the tympanum a certainty.[1] The left side of the palate is lax on phonation, and the voice perhaps a little rougher than normal. No difference in vibration could be made out by palpation over the crico-thyroid muscles during phonation. The patient faints occasionally, but these attacks are not preceded by cough, and their direct connexion with pressure on the pneumogastric, though probable, cannot be definitely established.

Before proceeding to discuss the detailed symptoms in this case, it may be well to state that the parotid tumour was eventually found to be an epithelial cancer attached to the base of the skull, and therefore irremediable. Looking at the case in the light of this fact, there can, I think, be little doubt that the various symptoms were due to pressure upon the hypo-glossal, glosso-pharyngeal, and pneumogastric nerves, together with those fibres of the spinal accessory which are incorporated with the vagus. Probably, as we shall see later, the sympathetic was also involved, but this is doubtful.

Cases similar to the one just described are very rare, although a few examples have been recorded. Before the use of the laryngoscope, instances of paralysis of both laryngeal nerves from malignant disease affecting the base of the skull were described by Dufour and Tuerk. In 1864 Gerhardt added another example; but the only fully described cases accessible to me were observed by Schech[2]

[1] Paracentesis was not performed; considering the serious nature of the case, I did not deem it right to add to the patient's discomfort.

[2] *Deutsches Archiv für Klin. Med.*, 1879, p. 157.

and Fraenkel.[1] Both patients showed nearly the same signs and symptoms as those just described. Fraenkel, however, noted the following additional points, which were not present in the case under consideration:—(1.) The tongue was atrophied as well as paralyzed; (2.) The paralyzed side of the epiglottis lay at a lower level than the other; (3.) The paralyzed cord was not in the middle line, but in the cadaveric position; (4.) The patient had to exercise great care in swallowing to avoid choking; (5.) The appetite was excessive, and the pulse varied from 110 to 120, symptoms which Fraenkel regarded as due to the pneumogastric paralysis; (6.) The pupils were unequal. In Schech's case the facial and fifth nerves were both temporarily affected; and although there was only pressure on one pneumogastric nerve, the cord of the opposite side was paretic,—otherwise the case is similar to mine.

I shall now consider some of the more prominent symptoms in the case described, considered from a physiological point of view.

(1.) *The presence of an abnormal quantity of mucus on the paralyzed side of the larynx and deeper part of the pharynx.*—This condition was due, I take it, not to any vaso-motor disturbance, but rather to the diminished sensibility of the parts and the interference with the nerve supply of the superior and middle constrictors of that side, which, as we now know, receive a considerable part of their innervation from the spinal accessory.[2]

(2.) *The position of the paralyzed cord and anæsthesia.*—This combination of symptoms pointed to pressure on the pneumogastric and accessory fibres, causing complete paralysis of sensation (or nearly so), but incomplete paralysis of motion; for it will be remembered that the cord of the affected side was in the middle line, the adductors being still active, but the abductor paralyzed. This fact also accounted for the remarkably good vocal power possessed by the patient. The flaccid condition of the epiglottis on the paralyzed side pointed to paresis of the thyro- and ary-epiglottic muscles or depressors of this cartilage, which are probably supplied by the superior laryngeal nerve. There was, moreover, no evidence of crico-thyroid paralysis (wavy outline of the glottis or absence of vocal power).

(3.) *The absence of all difficulty in swallowing* must, I think, be explained by the patient's having unconsciously acquired the power of guiding the food over the paralyzed side; otherwise it is difficult to account for the absence of this symptom, which was marked in Fraenkel's case.

(4.) The pressure on the hypoglossal nerve was evidently but slight, for the deviation from the middle line on protrusion of the

[1] *Berlin Klin. Wochenschrift*, 1875, No. 3.

[2] Schech, "Die Krankheiten der Mundhöhle, der Rachens und der Nase," 1885, p. 65.

tongue was just perceptible. On the other hand, the glosso-pharyngeal was more deeply implicated, as shown by the anæsthesia of the pharynx and loss of taste on the posterior part of the tongue. Taste was, however, preserved in the anterior portion of this organ; because, as is now well known, this region is furnished with fibres of special sense by the chorda tympani.

(5.) The paresis of the palate deserves some attention. It will be remembered that there was absolutely no evidence of implication of the facial nerve, and, indeed, such implication above its junction with the petrosal would be almost out of the question. In looking into the literature of the nerve supply of the palate, I was much struck by the fact that while most of our text-books put down the levator palati and azygos uvulæ as supplied by the facial alone, it seems quite understood in Germany that their motor power is derived both from the facial and spinal accessory. Those who are interested in this point will find it fully discussed in Schech's paper above referred to.

(6.) The last point to which reference need be made is the question of implication of the sympathetic. There was no congestion of the head, nor change in the pupil; the neuralgia was probably due to pressure of the tumour on the auriculo-temporal branch of the fifth. It will be remembered that there was congestion of the left tympanic membrane and serous exudation within the tympanum. On the other side there was chronic middle ear catarrh, with some obstruction of the Eustachian tube; and this was probably the initial condition on the left side also. The question seems to suggest itself, Whether in a commencing lesion of the cervical sympathetic a part under diminished atmospheric pressure would not tend to become affected by vaso-dilator influences much more readily than other regions? It may be that in this way the morbid condition of the middle ear is to be explained; it is also possible that the tumour pressed upon the tympanic veins, and thus produced a congestive serous exudation.

Mr Joseph Bell was much interested in Dr M'Bride's paper, inasmuch as it bore on a case which he had under his care during the last three months. It was one of sarcomatous tumour of the neck, and was chiefly remarkable because of its effects by pressure. Auditory, laryngeal, and tongue phenomena were set up in consequence. The patient became partially deaf, was at times unable to swallow, and had bad attacks of dyspnœa. The tumour was too far gone for removal; but a remarkable change took place in it. As the patient got weaker and thinner and lost ground it seemed to shrivel. This was a point he had observed in connexion with such tumours before, but, so far as he knew, there was no notice taken of it in the books. One day, after a dyspnœic attack, in which the patient was thought to be dying, an apparent improvement took place. The power of swallowing returned, and he never again had the

faintest trace of dyspnœa. He wished to ask Dr M'Bride if he knew of any similar case where apparent improvement followed on the disturbance caused by tumour pressure.

Dr M'Kenzie Johnston understood Dr M'Bride to say that in paralysis of the abductors there was no alteration in the voice. Now, in one of the cases of bilateral paralysis, that one which he had brought to Dr M'Bride, the first thing that attracted his attention was the change in the voice. Another point to which he wished to refer was the omission of any allusion to paralysis of both cords, resulting from pressure on one side only. He thought that one or two cases of this nature had been recorded.

Dr M'Bride thanked the Society for the kind attention given him during the reading of his paper. He was much interested in Mr Bell's case. He thought the apparent improvement might be quite consistent with an increase of pressure. As Dr Johnston had remarked, pressure on one nerve sometimes caused a reflex paralysis of the opposite cord. In such a case there might have been paralysis of abduction in the early stage, and a partial increase of pressure might have led to paralysis of both abduction and adduction. This was only a hypothesis, but it might help to explain the change. The case to which Dr Johnston had referred was one of bilateral paralysis, and he thought he had said that the voice might be slightly changed in that form of paralysis. It was in paralysis of one cord—a condition very frequently passed over—that there was no change in the voice. Dr Johnston had also referred to cases of bilateral paralysis from pressure on one recurrent nerve. Such cases certainly did occur, and their explanation was far from certain. It had been suggested that they were of the nature of a reflex paralysis. Probably they were rather due to an ascending neuritis affecting the nucleus of the other nerve. Another explanation that had been given was that where the paralyzed cord did not reach the middle line in phonation, the efforts of the other cord to cross over weakened and ultimately paralyzed it.

Meeting IX.—May 20, 1885.

Dr David Wilson, *Vice-President, in the Chair.*

I. Exhibition of Patients.

1. *Mr Joseph Bell* showed a patient suffering from a NÆVOID TUMOUR OF THE ARM AND SHOULDER. The patient, a man aged 27, was a signalman from Renfrewshire. The tumour appeared to be congenital, the patient stating that he was born with blue marks and swellings on his arm as at present. The amount of blood in

the limb made it feel very heavy, but on raising it the veins emptied themselves out of it. Beyond the weight and weakness of the limb the lesion caused no bad symptoms; nor did the sudden return of blood to the body from the emptying of the veins seem to give rise to any inconvenience, except a slight feeling of vertigo when he stooped. The condition for which he came to consult Mr Bell was a small tumour on the right side of the neck, of the exact nature of which he (Mr Bell) was uncertain. It lay partly under the right sterno-mastoid, was freely mobile, and became more prominent during the act of deglutition. He also thought it contained fluid. On two previous occasions it seemed to disappear under treatment. It was slightly painful, and once or twice he complained that it caused him some difficulty in swallowing. It had at no time interfered with his breathing, and a laryngoscopic examination had shown nothing abnormal in the upper respiratory passages.

2. *Mr Maxwell Ross* showed two cases in which tracheotomy had been performed by his colleague, Dr Hunter Mackenzie, for LARYNGEAL STENOSIS. The first case was a boy aged 6, on whom the operation had been performed for stenosis oocurring some months after an attack of measles. Of the exact cause of the stenosis they could not be quite certain, a laryngoscopic examination having failed to show all the parts clearly on account of the irritability present. So far as they could make out, the condition appeared to be one of warty growths. The tube was worn for about a year, during which time the larynx, having been left at rest, righted itself, and the stenosis disappeared. The voice, which had completely disappeared, returned, and the tube was able to be dispensed with. The second case was one of lupus of the larynx in a boy aged 18, whose face was similarly affected. The disease first showed symptoms of its presence in the beginning of last year. Tracheotomy was performed in October. The face was scraped with the sharp spoon. No bacilli were found in the scrapings. The larynx showed nodular thickenings, affecting especially the epiglottis. Since the operation no treatment had been adopted, excepting the administration of cod liver oil in large doses. Under this he had greatly improved, but could not yet dispense with the tube.

3. *Dr Allan Jamieson* exhibited a boy aged 12, who, since he was three years old, suffered from a RECURRENT VESICULAR ERUPTION, affecting solely the backs of the hands, the face, neck, and ears. From October till February of each year he remained free from attacks, and could attend school. In February the crops of vesicles began to appear, becoming larger and more numerous, and the outbursts more frequent as the weather became warmer, again to decline with the diminution of temperature. He felt chilly before an attack, his face swelled and grew red, and on this isolated

flat vesicles the size of a pea and larger came out. The centres of these were depressed and dark, the edges clear. Some vesicles dried up into thick crusts, others ruptured, and formed more extensive and black scabs. The usual duration of an outbreak was about three weeks, then there might be a period of intermission, or fresh vesicles might show themselves before the previous ones had finally disappeared. Smooth, white cicatrices were left by all or nearly all the vesicles, which were permanent. The disease caused no itching nor subjective sensation. It was not a lupus, as the lesions were superficial, and each self-limited. It bore more resemblance to impetigo or dermatitis herpetiformis, which recurred, but did not leave scars. No treatment had been of any avail so far. One medical man had recommended poultices of cow-dung, and bathing the parts with cow's urine. This repulsive method had been persevered in for some time without effect. The boy's father and mother were remarkably healthy, and so were nine brothers and sisters, six older and three younger. One brother, younger, had died of inflammation of the lungs.

II. Exhibition of Pathological Specimens.

1. *Mr Joseph Bell* showed an ARM and SCAPULA and CLAVICLE removed for SARCOMA from a boy aged 10. The tumour had been several months growing. The case was first under the care of a medical man in the north, who recognised its gravity. It, however, passed out of his hands into those of a female bone-setter, who did not improve matters by her attempt to set the so-called dislocation. The patient then returned to his first attendant, who sent him to the Infirmary to be under Mr Bell's charge. It was at once evident that nothing short of amputation of the arm, scapula, and greater part of the collar-bone, would be of the slightest use. The tumour involved both supra- and sub-scapular regions, and pressed on the axilla. It was also adherent to the skin of the back. The two chief difficulties of the operation were to prevent hæmorrhage, and get sufficient skin to cover in the parts left. The first difficulty was overcome by a method suggested by Mr Chiene, but slightly modified. Mr Chiene's idea was to pass a skewer from the anterior edge of the trapezius behind the subclavian, bringing it out through the pectorals, and compressing the vessels between it and an elastic band. As he had not done this before on the living body, he thought it would be safer to first make his section of the collar-bone, introduce his finger to feel the position of the vessels, and then pass the skewer through the wound guided by his finger. The disadvantage of this was that it did not command the supra-scapular and posterior scapular arteries as Mr Chiene's method would have done. Fortunately, these vessels were secured as soon as they came in view, and very little blood was lost. In addition to the skewer, he was fortunate in having Mr Duncan's

fingers compressing the vessels, so that had the skewer failed no accident would have happened. An incision was then made along the posterior border of the scapula, and the tumour, with the portion of skin adherent being cleared, was carried forward across the deltoid and round the arm. A flap of skin was dissected off the muscle, which, when split on the outer side, gave ready access to the axilla. The rest of the dissection was easy. The flaps came together readily. The edge of the lower had since died, but without putrefaction. The patient was doing well, and promised to make a good recovery.

Professor Chiene thought it right to say that Professor M'Leod of Calcutta had informed him he had adopted a somewhat similar method of controlling hæmorrhage in a case of amputation of the upper extremity and scapula for sarcoma. After division of the clavicle, a large acupressure needle was passed under the axillary vessels and brachial plexus, the point of it being raised and made to rest on the end of the divided clavicle. The case was fully reported in the *Indian Medical Gazette* for the year 1867. Subsequently, in the year 1880, previous to removing an immense venous hæmatoma of the axilla, Dr M'Leod succeeded in commanding the axillary vessels by passing a strong loop of catgut beneath them by means of a Wood's hernia needle. The loop was left *in situ*, after having been loosened for four days in case of secondary bleeding. This case was reported in the *Lancet*. In a more recent case of amputation of the upper extremity and scapula on account of a large sarcoma of the arm involving the shoulder-blade, the catgut loop plan was again tried, but the vessels had been displaced backwards by the tumour, and the loop was found to pass in front of them. These expedients could, of course, only control the axillary vessels.

Mr Miller asked why a curved skewer was preferred to a straight one

Professor Chiene said, that as the idea of using the skewer was his, he might answer Mr Miller's question. A straight skewer would have done well enough in an ordinary case. When passed from the anterior edge of the trapezius in such a direction as to command the transversalis colli and supra-scapular arteries, as well as the main subclavian trunk, it came out in the axilla. In Mr Bell's case the axilla was occupied in part by the tumour. It was therefore necessary to avoid this region, and he thought this would be best done by means of a curved instrument, which could be brought out in front through the pectorals.

2. *Mr Symington* showed a frozen section of PROLAPSUS UTERI.

3. *Dr Duncan* showed (*a*) a SCIRRHUS PROSTATE taken from a patient he had seen in consultation with Dr Shand. For some days previously there had been persistent hæmaturia. On exa-

mining the bladder one could feel above the pubis a certain hardness, and on putting the finger into the rectum a dense, hard prostate. The conclusion come to was that the tumour was malignant. Next day, as he was making no water, it was thought advisable to pass a catheter to make out whether this was due to suppression or retention. The passage of the instrument was attended with much difficulty. It was got into the prostate, but would go no further. After consultation with Dr Shand it was determined to force a passage. This was done, and about six ounces of bloody urine drawn off. The catheter was left in, but practically no more urine came away. The condition was one of suppression, what had been drawn off being merely residual urine. At the post-mortem examination the disease was found to have extended from the prostate on to the walls of the bladder in the form of nodules, which gave rise to the hæmorrhage. The passage made through the gland would be seen to the left of the urethra, about ¼ inch in length. (*b.*) The half of a LOWER JAW removed for malignant disease. The specimen presented a certain peculiarity. It was an epithelioma, and the floor of the mouth to the anterior pillar of the fauces had to be taken away because of the extension of the disease. The epithelioma had penetrated the bone by the socket of a tooth, eating its way into the interior, and then perforating on both sides as a sarcoma would do. The effect of this was that on proceeding to remove the jaw by the *par arrachement* method of Maisonneuve the bone fractured. The principle of this method was that to avoid section of the internal maxillary artery the fibres of the external pterygoid muscle, which alone remained attached to the jaw after division of the masseter, internal pterygoid, and temporal muscles, should be torn through by twisting of the jaw outwards. When this was done the fibres separated always at the same distance from the insertion. On this occasion although the bone fractured, thereby doing away with the leverage he ought to have had, he was nevertheless able to separate the muscle at the proper place—close to the neck.

Mr Bell said Mr Duncan's first specimen reminded him of one of the earliest cases of cancer of the prostate he had had under his care. It occurred in a young man whom several surgeons had been treating for retention. When admitted to hospital the patient was moribund. The catheter had to be passed once or twice, and it was a very easy matter to get the large silver instrument into the bladder. If it didn't go one way it was sure to go another. At the post-mortem a large encephaloid mass was found in the prostate, and through this four false passages made their way into the bladder.

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fingers compressing the vessels, so that had the skewer failed no accident would have happened. An incision was then made along the posterior border of the scapula, and the tumour, with the portion of skin adherent being cleared, was carried forward across the deltoid and round the arm. A flap of skin was dissected off the muscle, which, when split on the outer side, gave ready access to the axilla. The rest of the dissection was easy. The flaps came together readily. The edge of the lower had since died, but without putrefaction. The patient was doing well, and promised to make a good recovery.

Professor Chiene thought it right to say that Professor M'Leod of Calcutta had informed him he had adopted a somewhat similar method of controlling hæmorrhage in a case of amputation of the upper extremity and scapula for sarcoma. After division of the clavicle, a large acupressure needle was passed under the axillary vessels and brachial plexus, the point of it being raised and made to rest on the end of the divided clavicle. The case was fully reported in the *Indian Medical Gazette* for the year 1867. Subsequently, in the year 1880, previous to removing an immense venous hæmatoma of the axilla, Dr M'Leod succeeded in commanding the axillary vessels by passing a strong loop of catgut beneath them by means of a Wood's hernia needle. The loop was left *in situ*, after having been loosened for four days in case of secondary bleeding. This case was reported in the *Lancet*. In a more recent case of amputation of the upper extremity and scapula on account of a large sarcoma of the arm involving the shoulder-blade, the catgut loop plan was again tried, but the vessels had been displaced backwards by the tumour, and the loop was found to pass in front of them. These expedients could, of course, only control the axillary vessels.

Mr Miller asked why a curved skewer was preferred to a straight one

Professor Chiene said, that as the idea of using the skewer was his, he might answer Mr Miller's question. A straight skewer would have done well enough in an ordinary case. When passed from the anterior edge of the trapezius in such a direction as to command the transversalis colli and supra-scapular arteries, as well as the main subclavian trunk, it came out in the axilla. In Mr Bell's case the axilla was occupied in part by the tumour. It was therefore necessary to avoid this region, and he thought this would be best done by means of a curved instrument, which could be brought out in front through the pectorals.

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moribund, died shortly after. At the post-mortem examination seventeen false passages were found.

Mr Bell—All into the bladder?

Dr Duncan—No. I don't think any of them reached the bladder.

4. *Professor Chiene* showed a series of SARCOMATOUS TUMOURS of the lower extremity, from cases which had been under his care, illustrating the clinical aspects of the different degrees of malignancy. The first tumour was from a young man æt. 19, sent in by Dr Badger of Penicuik. The growth was of two months' duration, and involved the femur. Amputation at the hip was performed on 15th December 1884. He returned in April 1885 with pain in the stump, a bad cough, and died in the beginning of May. The post-mortem examination showed a number of osteo-sarcomatous tumours throughout the internal organs, with all the peculiarities of the first tumour. They were scattered very extensively throughout the lungs, and the right supra-renal capsule was one mass of sarcomatous tissue filled with blood. These, along with a section of the original tumour made by Dr Caird, were shown. The second tumour was a myeloid sarcoma of the thigh from a man aged 22, sent in by Dr MacNair of Falkirk. The tumour was of three months' duration. Amputation was performed on 20th May 1884. This patient died on the 3rd of August, after his return home. The cause of death was acute phthisis. The third was from a man aged 40, sent in by Dr Keith Anderson of Arbroath. The tumour had existed about nine months. It was a large spindle-celled sarcoma under the fascia, on the outer aspect of the thigh, and was removed on 30th January 1880. A small tumour was afterwards removed at home from the same situation. He returned with a third tumour in March 1881. Amputation at the hip was then performed. He recovered and went home, but died in the following year of phthisis in the Arbroath Infirmary. There was no post-mortem, and it was doubtful whether the symptoms were not rather due to recurrent sarcomata in the lungs than to real phthisis pulmonalis. The fourth was from a boy aged six, sent by Dr Macpherson of Bonar Bridge. It had existed eight months. Amputation at the hip was performed in January 1885. He had a note the other day to say the little fellow was now strong and well. The section prepared by Mr Mackay showed very well the connexion of the periosteum with the tumour. It was a round-celled sarcoma. These were cases in which they had or were liable to have recurrent tumours. He had had no case like Mr Spence's famous case of Mrs Easton, one of whose tumours he had the pleasure of showing, but he had had three cases which were somewhat similar to two other historical cases, one of which was under the care of Mr Goodsir (see Spence's *Surgery*). These three were cases of spindle-celled sarcomata, and might be con-

sidered as typically recurrent cases. The first occurred in a woman aged 55, who appeared to be otherwise in the enjoyment of good health. In her case there was a history of injury—a blow from a mangle five months before her admission to the Infirmary. The tumour, which was about the size of an orange, occupied a position on the outer side of the thigh, and was connected with the fascia above the knee-joint. There was a wound in it, made by a surgeon who had lanced it. Nothing but blood escaped. It was a typically soft tumour, with a great tendency to hæmorrhage, and was removed in June 1879. She returned in June 1881 with a tumour the size of a hen's egg; in January 1883 with a trilobate tumour of the size of a small orange; in May 1883 with a tumour which was shown. These four tumours, removed between 1879 and 1883, were all taken from the outer aspect of the thigh. In June 1883 a firm, hard tumour, non-glandular, was removed from the groin by Mr Hare. In November 1883 another tumour was removed from the same situation. It was about the size of a man's fist, and the femoral sheath was exposed in the dissection necessary for its removal. It was softer than the earlier tumours, and Prof. Greenfield found it was undergoing myxomatous degeneration. In June 1884 a third tumour appeared in the groin, and for the first time the glands were also affected. Both tumour and glands were removed. The wound healed without pain or discomfort. In August she began to complain of pain in the liver, and Dr MacGillivray found that organ enlarged. She died in September, and at the post-mortem examination there were found sarcomatous nodules in the cicatrix, in the heart, lungs, liver, and lumbar region. He showed the heart, with nodules in its substance, and a pedunculated tumour projecting into the right ventricle. The second of these cases occurred in a man aged 48, sent to the hospital by Dr Porteous, Pathhead. He was in typically good health, and in his case also there was a history of injury —a blow from a cask on the calf, which caused a rather severe bruise. The tumour appeared some weeks afterwards at a lower part of Scarpa's triangle. In June 1879 the tumour was about the size of a small orange, and firm in consistence. This was removed, but recurred in February 1880. The tumour removed on this occasion was shown. In June 1880 a third tumour, hard, and about the size of a marble, was removed from over the adductor longus. At this time the glands were unaffected. In October 1881 he returned with a tumour of the size of a goose's egg, hard, and attached to the femoral vein, which required ligature. In March 1882 a tumour of the size of a small orange was taken away, and in November 1882 two tumours from the same region. A year later—November 1883—he came back with a tumour, which required an incision five inches in length for its removal. This made the seventh operation performed on this man's groin. In 1884 he returned with a tumour, which was not removable on

probe shows that the parts on the left side can be touched without producing either sensation or reaction—*e.g.*, epiglottis, region of the arytenoids, and pyriform sinus. A similar proceeding attempted on the other side at once causes cough, vomiting, and spasm. There was nothing abnormal noticed about the epiglottis, except that when its left margin was hooked upon the probe it was noticeably yielding and flabby—*i.e.*, compared with the resistance offered in a healthy larynx. There is very slight difficulty in swallowing, but it does not cause discomfort. The left faucial pillar is quite anæsthetic, and touching it produces no reaction. When the tongue is protruded, it is pushed a little to the left. Taste is absent posteriorly over the left side of this organ (the substances used being sweet, salt, and bitter), but is preserved anteriorly. The appetite is moderate. The pulse is not much quickened, and the eyes normal. There is neuralgia of the left ear and side of the head, and the patient states that he feels an impression passing from the left ear to the throat. There is no congestion of the side of the head. In the right ear there is evidence of chronic middle ear catarrh; while on the left side the handle of the malleus is congested, and the yellow discoloration of the membrane at its posterior inferior part, together with a distinctly visible line of demarcation, renders the presence of serous exudation within the tympanum a certainty.[1] The left side of the palate is lax on phonation, and the voice perhaps a little rougher than normal. No difference in vibration could be made out by palpation over the crico-thyroid muscles during phonation. The patient faints occasionally, but these attacks are not preceded by cough, and their direct connexion with pressure on the pneumogastric, though probable, cannot be definitely established.

Before proceeding to discuss the detailed symptoms in this case, it may be well to state that the parotid tumour was eventually found to be an epithelial cancer attached to the base of the skull, and therefore irremediable. Looking at the case in the light of this fact, there can, I think, be little doubt that the various symptoms were due to pressure upon the hypo-glossal, glosso-pharyngeal, and pneumogastric nerves, together with those fibres of the spinal accessory which are incorporated with the vagus. Probably, as we shall see later, the sympathetic was also involved, but this is doubtful.

Cases similar to the one just described are very rare, although a few examples have been recorded. Before the use of the laryngoscope, instances of paralysis of both laryngeal nerves from malignant disease affecting the base of the skull were described by Dufour and Tuerk. In 1864 Gerhardt added another example; but the only fully described cases accessible to me were observed by Schech[2]

[1] Paracentesis was not performed; considering the serious nature of the case, I did not deem it right to add to the patient's discomfort.

[2] *Deutsches Archiv für Klin. Med.*, 1879, p. 157.

and Fraenkel.[1] Both patients showed nearly the same signs and symptoms as those just described. Fraenkel, however, noted the following additional points, which were not present in the case under consideration:—(1.) The tongue was atrophied as well as paralyzed; (2.) The paralyzed side of the epiglottis lay at a lower level than the other; (3.) The paralyzed cord was not in the middle line, but in the cadaveric position; (4.) The patient had to exercise great care in swallowing to avoid choking; (5.) The appetite was excessive, and the pulse varied from 110 to 120, symptoms which Fraenkel regarded as due to the pneumogastric paralysis; (6.) The pupils were unequal. In Schech's case the facial and fifth nerves were both temporarily affected; and although there was only pressure on one pneumogastric nerve, the cord of the opposite side was paretic,—otherwise the case is similar to mine.

I shall now consider some of the more prominent symptoms in the case described, considered from a physiological point of view.

(1.) *The presence of an abnormal quantity of mucus on the paralyzed side of the larynx and deeper part of the pharynx.*—This condition was due, I take it, not to any vaso-motor disturbance, but rather to the diminished sensibility of the parts and the interference with the nerve supply of the superior and middle constrictors of that side, which, as we now know, receive a considerable part of their innervation from the spinal accessory.[2]

(2.) *The position of the paralyzed cord and anæsthesia.*—This combination of symptoms pointed to pressure on the pneumogastric and accessory fibres, causing complete paralysis of sensation (or nearly so), but incomplete paralysis of motion; for it will be remembered that the cord of the affected side was in the middle line, the adductors being still active, but the abductor paralyzed. This fact also accounted for the remarkably good vocal power possessed by the patient. The flaccid condition of the epiglottis on the paralyzed side pointed to paresis of the thyro- and ary-epiglottic muscles or depressors of this cartilage, which are probably supplied by the superior laryngeal nerve. There was, moreover, no evidence of crico-thyroid paralysis (wavy outline of the glottis or absence of vocal power).

(3.) *The absence of all difficulty in swallowing* must, I think, be explained by the patient's having unconsciously acquired the power of guiding the food over the paralyzed side; otherwise it is difficult to account for the absence of this symptom, which was marked in Fraenkel's case.

(4.) The pressure on the hypoglossal nerve was evidently but slight, for the deviation from the middle line on protrusion of the

[1] *Berlin Klin. Wochenschrift*, 1875, No. 3.

[2] Schech, "Die Krankheiten der Mundhöhle, der Rachens und der Nase," 1885, p. 65.

tongue was just perceptible. On the other hand, the glosso-pharyngeal was more deeply implicated, as shown by the anæsthesia of the pharynx and loss of taste on the posterior part of the tongue. Taste was, however, preserved in the anterior portion of this organ; because, as is now well known, this region is furnished with fibres of special sense by the chorda tympani.

(5.) The paresis of the palate deserves some attention. It will be remembered that there was absolutely no evidence of implication of the facial nerve, and, indeed, such implication above its junction with the petrosal would be almost out of the question. In looking into the literature of the nerve supply of the palate, I was much struck by the fact that while most of our text-books put down the levator palati and azygos uvulæ as supplied by the facial alone, it seems quite understood in Germany that their motor power is derived both from the facial and spinal accessory. Those who are interested in this point will find it fully discussed in Schech's paper above referred to.

(6.) The last point to which reference need be made is the question of implication of the sympathetic. There was no congestion of the head, nor change in the pupil; the neuralgia was probably due to pressure of the tumour on the auriculo-temporal branch of the fifth. It will be remembered that there was congestion of the left tympanic membrane and serous exudation within the tympanum. On the other side there was chronic middle ear catarrh, with some obstruction of the Eustachian tube; and this was probably the initial condition on the left side also. The question seems to suggest itself, Whether in a commencing lesion of the cervical sympathetic a part under diminished atmospheric pressure would not tend to become affected by vaso-dilator influences much more readily than other regions? It may be that in this way the morbid condition of the middle ear is to be explained; it is also possible that the tumour pressed upon the tympanic veins, and thus produced a congestive serous exudation.

Mr Joseph Bell was much interested in Dr M'Bride's paper, inasmuch as it bore on a case which he had under his care during the last three months. It was one of sarcomatous tumour of the neck, and was chiefly remarkable because of its effects by pressure. Auditory, laryngeal, and tongue phenomena were set up in consequence. The patient became partially deaf, was at times unable to swallow, and had bad attacks of dyspnœa. The tumour was too far gone for removal; but a remarkable change took place in it. As the patient got weaker and thinner and lost ground it seemed to shrivel. This was a point he had observed in connexion with such tumours before, but, so far as he knew, there was no notice taken of it in the books. One day, after a dyspnœic attack, in which the patient was thought to be dying, an apparent improvement took place. The power of swallowing returned, and he never again had the

faintest trace of dyspnœa. He wished to ask Dr M'Bride if he knew of any similar case where apparent improvement followed on the disturbance caused by tumour pressure.

Dr M'Kenzie Johnston understood Dr M'Bride to say that in paralysis of the abductors there was no alteration in the voice. Now, in one of the cases of bilateral paralysis, that one which he had brought to Dr M'Bride, the first thing that attracted his attention was the change in the voice. Another point to which he wished to refer was the omission of any allusion to paralysis of both cords, resulting from pressure on one side only. He thought that one or two cases of this nature had been recorded.

Dr M'Bride thanked the Society for the kind attention given him during the reading of his paper. He was much interested in Mr Bell's case. He thought the apparent improvement might be quite consistent with an increase of pressure. As Dr Johnston had remarked, pressure on one nerve sometimes caused a reflex paralysis of the opposite cord. In such a case there might have been paralysis of abduction in the early stage, and a partial increase of pressure might have led to paralysis of both abduction and adduction. This was only a hypothesis, but it might help to explain the change. The case to which Dr Johnston had referred was one of bilateral paralysis, and he thought he had said that the voice might be slightly changed in that form of paralysis. It was in paralysis of one cord—a condition very frequently passed over—that there was no change in the voice. Dr Johnston had also referred to cases of bilateral paralysis from pressure on one recurrent nerve. Such cases certainly did occur, and their explanation was far from certain. It had been suggested that they were of the nature of a reflex paralysis. Probably they were rather due to an ascending neuritis affecting the nucleus of the other nerve. Another explanation that had been given was that where the paralyzed cord did not reach the middle line in phonation, the efforts of the other cord to cross over weakened and ultimately paralyzed it.

Meeting IX.—May 20, 1885.

DR DAVID WILSON, *Vice-President, in the Chair.*

I. EXHIBITION OF PATIENTS.

1. *Mr Joseph Bell* showed a patient suffering from a NÆVOID TUMOUR OF THE ARM AND SHOULDER. The patient, a man aged 27, was a signalman from Renfrewshire. The tumour appeared to be congenital, the patient stating that he was born with blue marks and swellings on his arm as at present. The amount of blood in

order, were recorded on a blackened revolving cylinder by the following very simple method:—

This method consisted in introducing into the air-passages a narrow curved glass tube (internal diameter 1 mm.), bearing on its lower end a small bag of animal membrane, made from the intestine of a rabbit, the only opening into which was through the glass tube. This tube was passed downwards through an opening in the trachea until, by a finger passed into the opening in the thorax, it was felt that the point of the tube reached about the middle of the lower lobe of one lung (usually the right). The upper end of the glass tube was then connected with the recording apparatus which Professor Roy has elsewhere[1] described as the oncograph, and which is capable of recording very minute changes of pressure. The whole apparatus was filled with air, instead of oil or water, in order to reduce friction.

But while this method proved to be more than sufficiently delicate for such work as ours, it possessed certain sources of possible error which we sought to avoid in various ways. One of them was, that the presence of the bladder in the bronchus prevented the access of air to the lung tissue lying beyond. This can hardly, however, affect the bronchus, the blood supply of which is distinct from the pulmonary vessels, nor as a matter of fact did that obstruction produce any deleterious influence upon the aeration of the blood, the other portions of lung tissue being amply sufficient for the requirements of the animal. The chief objection to the method, however, consists in the fact that unless the pressure in the bladder be higher than the maximum pressure of the air forced in by the artificial respiration apparatus, the lever records these respirations. To have the pressure high would lead to local distension of the bronchus, which we wished to avoid. This difficulty does not affect the position of the lever during expiration, when the air-pressure is far below that in the bladder, so that it only makes the curves somewhat more complicated. This source of error can always be temporarily eliminated by stopping the respirations for a moment; and we have recently been able to improve our method so as to do away with it altogether.

The following facts we have good reason to believe to be trustworthy. Other results, about which we have some remaining doubt, we leave over for future control.

1. In the curarized dog, stimulation of one uncut vagus by means of an induced current causes well-marked contraction of the bronchi of both lungs. Certain exceptions to this we will presently allude to.

2. Section of one vagus usually causes a marked expansion of the bronchi of the corresponding lung, which may be preceded by a slight temporary contraction, due apparently to the stimulus of the section.

[1] *Journal of Physiology*, vol. iii. No. 3, and Virchow's *Archiv*, vol. xxii. 1883.

3. Stimulation of the peripheral end of one cut vagus always causes a much more powerful contraction than that which results from the application of the same strength of stimulus to the uncut nerve.

4. Stimulation of the central end of one cut vagus (the other being intact) usually causes contraction, but never so powerfully as when the peripheral end is stimulated.

5. Frequently, and especially in non-curarized animals narcotized with ether, stimulation of the central end of one cut vagus (the other being intact) causes a marked expansion of the bronchi.

6. In these animals stimulation of one uncut nerve (the other being cut) often causes a marked expansion of the bronchi. In the same cases stimulation of the peripheral end of the cut vagus causes a primary contraction, followed, during the stimulation, by an expansion which may dilate the bronchus beyond the calibre it showed before stimulation was commenced.

7. The extent of these changes in calibre varies greatly. On stimulation of the peripheral end of the vagus the bronchi often appear to close almost completely, and contractions equal to $\frac{1}{2}$ or $\frac{1}{3}$ of the previous diameter as frequently met with.

8. The expansions met with are, under certain conditions, as marked in extent as the contractions.

9. The above facts indicate that the vagi contain centripetal fibres which can cause both contraction and expansion. In some cases the latter effect predominates.

10. The marked difference in the effect of stimulation of the uncut nerve and the peripheral end of the cut nerve may be explained by the inhibiting action of the centripetal fibres on the upper centre or centres.

11. The fact that both contraction and expansion of the bronchi may be obtained by reflex from the cerebro-spinal system might be supposed to be due to the presence there of one centre, from which a tonus is constantly passing to the bronchial muscles, and the action of which centripetal stimuli inhibited; but we have not been able to convince ourselves that the readiness with which expansion is produced by central stimulation goes hand in hand with the amount of tonus indicated by the degree of expansion on cutting both vagi, as it ought to do were that hypothesis correct. Moreover, the double effect mentioned in paragraph 6 can scarcely be explained otherwise than by the presence in the vagus of both constricting and inhibiting fibres. This would of itself imply that there are two centres in the cerebro-spinal tissue which can influence the calibre of the bronchi in these directions either reflexly or by direct stimulation.

12. The facts noted in paragraph 6 indicate that the vagi contain both contracting and expanding fibres for the bronchi, the expanding being stimulated more slowly than the contracting, so that primary contraction is followed by secondary expansion, a

difference in excitability comparable with that found in the inhibiting and accelerating nerves of the heart.

13. Whether this would indicate that there are one or even two local centres in the lungs, or whether it can be explained by assuming that both contracting and inhibiting fibres terminate directly in the muscle fibres of the bronchi, we have not yet been able to decide. It is well known, however, that surrounding the bronchi are many ganglionic masses.

14. Asphyxia produced by stopping the respirations usually causes contraction of the bronchi, but in etherized dogs we have frequently found it to cause expansion, neither effect corresponding in time with the changes in the arterial blood pressure. After section of both vagi asphyxia produces no effect upon the calibre of the bronchi.

15. Stimulation of the central end of a sensory nerve, other than the vagus, was usually without effect on the bronchi. Very rarely, however, we did obtain a slight contraction on such stimulation.

16. Stimulation of stomach, pleura, tracheal mucous membrane, intestine, nostrils, etc., by means of an induced current, has not as yet been found by us to influence the calibre of the bronchi. It may be added, that after section of both vagi stimulation of their central ends in the neck produced no effect on the bronchi, showing that in the dog the only connexion between the cerebro-spinal centres and the bronchi is through these nerves.

17. In conclusion, we may refer to a few of the drugs, the action of which we tested. The effects of ether have been already noted in the preceding paragraphs. With it there is no paralysis of the contracting fibres, but under its influence we have expansion produced, apparently by stimulation of the inhibitory or expanding fibres. Whether it affects the cerebro-spinal centres or the terminations of the nerves in the bronchi we have not yet been able to decide.

Atropine, on the other hand, causes a complete paralysis of the constricting nerve fibres, or of their peripheral apparatus in the bronchi, so that, after a very small dose, stimulation of the peripheral end of the cut vagus, which previously caused strong contraction, has no longer any effect.

Nicotine, in small doses, has a powerful effect in expanding the bronchi, but its mode of action, as well as that of other drugs which affect the calibre of the bronchi, must be left to be mentioned elsewhere.

We also wish to reserve what we have to say on the question as to whether the bronchial contractions are vermicular, or are ever rhythmic, as we have some reason to believe they occasionally are.

I deem it unnecessary to point out to the Society the important bearing of the foregoing facts upon the pathology and treatment of many respiratory disorders, and particularly asthma. To this subject I shall probably refer on a future occasion.

Meeting X.—June 3, 1885.

Dr H. D. Littlejohn, *President, in the Chair.*

Original Communication.

ON THE DANGER OF NEGLECTING, AND ON THE BEST MEANS OF CONTROLLING THE FEBRILE STATE.

By T. M'Call Anderson, M.D., F.F.P.S. Glasg., Professor of Clinical Medicine in the University of Glasgow.

It cannot be denied that even at the present day, in dealing with fevers and inflammations, far too little attention is bestowed upon the element of fever; for it should never be forgotten that fever is of itself, either from its intensity or from its long continuance, a source of great danger, apart altogether from the cause which has induced it. And I have no hesitation in expressing the opinion that thousands of our countrymen are sacrificed annually owing to a neglect, or to a depreciation of the element of fever. This is especially the case with puerperal, scarlet, typhoid, and rheumatic fevers, and with acute phthisis.

No more serviceable nor more generally employed instrument than the clinical thermometer has come into use in the practice of medicine in recent years, but of how little value is it to the unfortunate patient if its readings are limited to aiding our diagnosis, or to giving an opinion as to the probable upshot of the complaint, as I have so frequently witnessed.

Fever is a consuming fire which, so long as it lasts, is accompanied by progressive emaciation and by increasing debility, so that as a rule it demands not inaction, on the lines of the old dogma that fever is an effort of nature to free the system from the noxious elements produced by the disease, and is therefore to be encouraged; not the use of so-called antiphlogistic remedies, but the employment of supporting measures. Of all the wise sayings of the late Dr Graves, perhaps the most sage was his remark—"If you are at a loss for an epitaph to be placed on my tomb, here is one for you: 'He fed fevers.'"

There are probably few at the present day who do not more or less act upon this principle, and who would not admit that the more intense and persistent the fever, the greater is the necessity for the administration of stimulants, given in small doses and at short intervals. But if a patient in danger of death from pyrexia must be well fed and freely stimulated, he must also be otherwise well nursed; indeed, the services of a trained and sagacious nurse are a *sine quâ non* in the management of such perilous cases.

These measures are of themselves calculated not only to mitigate

the ravages of the fever, but also to a certain extent to lower its intensity; but although indispensable to the maintenance of life, they cannot alone be relied upon to bring down the temperature in serious cases.

As you are well aware, the credit of having introduced the cold water treatment of fevers—chiefly in the shape of cold affusion—is due to James Currie, the founder of hydropathy towards the close of last century; but while hydropathy has flourished more and more since his day, the hydropathic treatment of pyrexia was soon forgotten, partly from the dread of an outraged public opinion, and partly because the wished-for result was not obtained, owing to its not having been used with sufficient energy and persistence, or with an abiding faith in its efficiency.

Cold, in some shape or other, is the most natural, and at the same time one of the most powerful measures at our disposal for reducing the temperature, although we must remember that the cooling down of the living body is a much more complex problem than that of cooling a body not endowed with life. This is apparent if we study the effects of the cold bath on a healthy person. The body protects itself in two ways: in the first place the cutaneous capillaries contract, thus limiting the abstraction of heat, and in the second there is a greatly-increased production of heat, so that although after the bath a slight cooling of the body takes place, during it (if it is not too long continued, and if the water is not too cold, thus putting too great a strain upon the regulating apparatus) there is no lowering, but even a slight rise of temperature. It is the same with a fever patient, with this exception, that the regulating power is not so active, and thus he is not so able to resist the cooling process. But still it is the persistent power of the fever patient to keep up his high temperature, just as a healthy person tends to keep up his normal temperature, which is the chief difficulty in the way of treatment.

Some benefit is to be derived from allowing our patient to suck ice *ad libitum;* from icing his food and drink; from sponging the body frequently with iced water or rubbing it with lumps of ice; and in certain cases Liebermeister's suggestion may be tried, viz., that of washing out "the intestinal mucous membrane for a length of time with a constant stream of cold water, by means of a double-action œsophageal tube carried far up the rectum, one pipe of which should be put in communication with the reservoir." In other cases cold affusion may be tried—although patients often object to it—and this may be done by bringing the head beyond the edge of the bed and pouring a quantity of cold water upon it. A more effectual method of treatment is the cold pack; and it has been estimated that three or four packs, each of half an hour's duration, is equivalent to a cold bath of ten minutes. Another useful remedy is the application of iced cloths to the abdomen, as recommended by me some years ago in the treatment of acute phthisis,

and which may be continued for half an hour or more at a time, the process being repeated as often as required, more especially if it is agreeable to the patient, who sometimes asks for its repetition.

The application of iced cloths is made in this way. The night dress is pulled well up over the chest, so as to avoid any possibility of its being wet; and, for a similar reason, a piece of Macintosh is placed across the bed under the patient's body, and another piece between the iced cloth and the bed-clothes. The usual bed-clothes are arranged so that they reach up to the lower part of the chest only, which latter is covered with a separate blanket in order to prevent unnecessary exposure while the iced cloths are being changed. Two pieces of flannel are employed in the process, each being sufficiently large, when folded into four layers, to cover the whole front and sides of the abdomen. One of these, wrung out of iced water, is applied, while the other is left in a large basin filled with iced water at the side of the bed. The pieces of flannel are changed every minute, or so often that *they still feel cold* when removed. The changing of the flannel, especially if two persons are in attendance,—one to remove the bed-clothes and the flannel, the other to apply the piece which is freshly iced,—can be effected with great ease and rapidity, and without exposing the patient to any injurious extent, if the preliminary arrangement of the bed-clothes is made in the way I have indicated. I have thought it right to mention these apparently trivial details, because I have often seen the process carried out in such a way as to be perfectly futile, and because I have frequently been interrogated on the subject.

The following case of acute phthisis illustrates this method of treatment, as well as other measures relied upon by me for the occasional cure of that disease:—

A carter, 25 years of age, whose family history was not unfavourable, but who was very intemperate in his habits, although he had never previously had an "hour's sickness," was admitted into the Western Infirmary of Glasgow on 20th May 1879, having then been ill for four weeks.[1] This illness set in on a Sunday morning with a feeling of sickness, which caused him to return to bed. In the evening he began to cough, and expectorated blood at first black and clotted, he said; afterwards, bright red. The hæmorrhage continued for four days, but in greatly diminished quantity, and consisted more of a blood-stained spit than of unmixed blood. The cough continued severe all this time, but disappeared in great measure with the hæmoptysis, about the fourth or fifth day. The day after the onset of the hæmorrhage he experienced severe pain in the right side of the chest, referred to

[1] For other cases, see the Author's *Lectures on Clinical Medicine, delivered in the Royal and Western Infirmaries of Glasgow*, p. 156. London: Macmillan & Co.

the mammary and suprascapular regions, aggravated by coughing and by lying on the right side. A week after he lay down the bowels began to be troublesome, moving frequently as often as five times in the twenty-four hours; the stools being thin and yellowish in colour, but not very copious. They had continued in this state when he entered the Infirmary.

Examination on Admission.—The patient was much emaciated, and very weak and exhausted. The skin was dry and hot, with perspirations at night, and temperature 103°; pulse 110, feeble; respirations 40, shallow and noisy; pulse-respiration ratio $2\frac{3}{4}:1$. He had occasional slight cough, with scanty clear mucous expectoration. Decubitus was dorsal. He had a slight flush on each cheek, and slight lividity of the lips. The alæ nasi dilated on inspiration. The tongue was slightly coated and moist. He had anorexia. The bowels were loose. There was no distension of the abdomen; no gurgling or tenderness in either iliac region; no eruption. The urine was 32 ounces, high coloured, acid, of specific gravity 1025; it contained a trace of bile, but no albumen. At the base of the right lung there were signs of moderate pleuritic effusion. At the left apex (especially in the supra-clavicular and infra-clavicular regions) were defective movements, slight flattening, moderate dulness. Moist (subcrepitant), mixed now and then with dry, *râles* were heard, and there was increased fremitus and resonance. At the base of the same lung posteriorly there was marked dulness, with very coarse crackling *râle*, and increased fremitus and resonance; over the rest of the lung there was a general want of resonance, with a mixture of dry and moist (subcrepitant) *râles*. There was no evidence of disease in any other organ.

Treatment.—*26th May.*—He had milk or soup every hour, the latter being avoided so long as there was looseness; also quinine one grain, digitalis half a grain, opium half a grain, every four hours. The opium in pills was increased on the 30th to three-fourths of a grain, on account of the looseness; but on 10th June, as he was sweating a good deal, as the other symptoms were moderating, and as there had been no motion for four days, the pills were diminished to three daily.

On *28th May* iced cloths were applied to the abdomen for half an hour every two hours (it was afterwards ascertained that they had not been thoroughly used until the evening of 4th June), after which the temperature fell more decidedly. In the later stages they were omitted whenever the temperature was below 100°. At the same date, two drachms of brandy every three hours were prescribed.

30th May.—Subcutaneous injections of atropia each night were commenced—$\frac{1}{100}$ of a grain, gradually increased to $\frac{1}{70}$. Eight times during the course of the illness a dose of 10 grains of quinine was given when the temperature tended to be high, which

was generally in the afternoon or evening; and occasionally the body was sponged with iced vinegar and water.

The patient's improvement under this treatment was steady and continuous. On 10*th June* it was recorded that his appetite and general condition were much improved; bowels not moved for four days; pulse 80 and of fair strength; respirations 24; temperature during preceding twenty-four hours never up to 100° F.; cough less, expectoration scanty, and only clear mucus. Coarse *râles* at left base gone; replaced by *râles* partly musical and partly moist. The pill had a markedly diuretic effect.

On 2*nd July* it was noted that the expectoration was still present and yellower, but scanty; digestive organs natural, but motions still inclined to looseness; temperature normal; pulse 86 and of good strength; respirations 20-24. All *râles* had disappeared from left side of chest, except occasional musical rhonchi posteriorly. Diet good, including beef-steaks.

1*st August*.—Diarrhœa and sweats gone; pulse 84; respirations 18; expectoration scanty, muco-purulent; *râles* have completely disappeared from chest; percussion-dulness less pronounced; there is still slight pleuritic effusion at right base. Takes cod-liver oil, ʒvj. per day. 14*th August*.—Percussion-dulness very decidedly diminished. 29*th August*.—Is practically convalescent; no cough or expectoration; temperature normal, respirations 16; no trace of *râle* in any part of chest; pleuritic effusion at base steadily diminishing; at left base the respiratory murmur is still somewhat feeble, and in the left infra-clavicular region there remains flattening, defective resonance, and harsh breathing; there is also some dulness in the supra-scapular region. From 10*th August* to 6*th October* he gained 8½ lbs. in weight.

There can be no doubt, however, that the most certain and speedy manner of reducing the temperature by cold is by the use of the cold bath. In cases of great exhaustion this procedure involves considerable responsibility, because the slight fatigue involved in putting a patient into and taking him out of a bath may prove the last straw which determines a fatal issue from syncope, and because it is apt to appear to the patient's friends to be a very heroic remedy. But although this last consideration need not weigh with us if there seems thereby a chance of saving life, I am inclined to think that the medicinal treatment of hyperpyrexia, in combination with some of the milder methods of applying cold, will come more and more to supplant the use of baths, especially as we are almost daily becoming acquainted with new drugs which act with nearly mathematical precision in the way of lowering the temperature of the body. At the same time, I must admit that I have experienced most brilliant results from the use of the bath, and one case I shall never forget—that of a young governess labouring under typhoid fever of a severe type,

with a temperature of above 105° F. at the end of the third week of her illness. She had profuse diarrhœa, hæmorrhage from the bowels, pronounced hypostatic congestion of the lungs, and she was almost insensible and pulseless. In this case, in addition to giving quinine in large doses, she was put into a cold bath, after which she almost immediately rallied, and although it was some days before the temperature became permanently normal, she made an excellent recovery.

As regards so-called antipyretic medicines, time will only permit of my referring to four—(1.) Salicine and the Salicylates, (2.) Quinine, (3.) Kairin, and (4.) Antipyrin.

Salicine and the salicylates have been much vaunted of late as antipyretics. Thus Dr Lauder Brunton, in his valuable work on Pharmacology, Therapeutics, and Materia Medica, just published, says of salicine, p. 939, "It is an antipyretic;" and of salicylic acid, p. 741, "It is a most powerful agent in lowering the temperature in fever." Now, every one will admit that, in cases of rheumatic fever, these drugs generally speedily bring down the temperature to the normal; but why do they do so? Not because they act upon the heat centre, but because they remove the rheumatism which is the cause of the fever. As a matter of fact, they cut short the febrile movement just as the surgeon's knife does when he opens an acute abscess and gives free exit to the pent-up pus.

But further, you will sometimes find that even although salicine puts a stop to the pain and swelling of the joints, it does not of necessity control the fever. This is well illustrated by a case reported by my colleague Professor Gairdner, of which the following is an outline[1]:—

A female, 35 years of age, was admitted into the Western Infirmary, 4th April 1877, with acute rheumatism of six days' duration, attributed to getting her feet wet. There was a history of a similar attack nine years before. All the joints, except the hips and shoulders, were swollen and painful, and there was pericarditis with evident effusion, accompanied by pain in the præcordial region and headache. On the night of admission the temperature was 103°·8 F., and salicine in 20-grain doses was given every hour, but was changed after three or four doses to salicylate of soda; and although, with the exception of severe headache, all pain had completely subsided by mid-day of the 5th (24 hours after admission), the temperature rose rapidly till it culminated on April 6th in 106°·2 F. Salicine was pushed in large and frequent doses to watch its effect on the temperature; but as this gradually rose to hyperpyrexia, the treatment by means of iced-cloths to the abdomen, above referred to, was employed, with the effect of bringing down the temperature rapidly, and keeping it perfectly under control. She had in all 600 grains of salicine, and 200 of salicylate of soda.

[1] *Glasg. Med. Journ.*, Oct. 1877, p. 435.

In a paper recently published by Dr Holland of St Moritz, giving cases of phthisis treated by means of antipyrin, I find that in six of them the salicylate of soda was tried, and in not a single case was the temperature lowered in the least. To tell the truth, I have no faith in the salicylates as antipyretics except to the extent already indicated, and I am quite willing to have the matter tested, and to compare their effects with those of the other medicines about to be mentioned, if a case—such as typhoid fever or acute phthisis—is selected, in which all chance of a rheumatic element is excluded.

But, while sceptical as to the antipyretic virtues of the salicylates, I have formed a very different opinion with regard to quinine, which I was taught in my student days to avoid sedulously in all cases in which fever is present—though, curiously enough, an exception was made in favour of intermittent fever. It is unnecessary for me to dwell upon this subject, because probably all will admit that quinine is a powerful antipyretic, if given in large doses (10 to 40 grains in a single dose, or in divided doses within an hour). In fact we must, as Liebermeister has remarked, give such a dose as will bring down the temperature for a time to the normal, on the principle that an extremely violent fever, which has occasional intermissions, is much less dangerous than a less violent fever which is continuous, or only shows slight remissions.

The other remedies to which I refer in conclusion have only been employed for a very short time, so that their virtues as antipyretics have not yet been so universally recognised, although in some respects they are decidedly superior to quinine—these are kairin and antipyrin.

Kairin is an artificial alkaloid recently built up synthetically by Dr Otto Fischer of Munich, and is described as being the hydrochlorate of oxy-chinilin-ethyl. This (and other similar bodies) was handed over to Dr Filehne for examination in 1881, who found it a most powerful antipyretic. The dose is 8 to 16 grains every hour until the temperature is reduced nearly to the normal, and generally three hourly doses are sufficient. As the temperature falls the pulse and respiration become slower, and there is generally profuse sweating, which, however, ceases when the temperature is normal. The sweating may usually be prevented by administering an anti-sudorific about a quarter of an hour before the medicine is commenced, such as a pilule of agaricin (gr. $\frac{1}{10}$) (Riegel), or (gr. $\frac{1}{60}$) of atropia. As the temperature falls the patient feels more comfortable, and there is never any really bad symptom unless the drug is impure, in which case cyanosis and collapse have been known to result. But within twelve hours of the commencement of the kairin the urine becomes greenish in colour, and this continues for about twenty-four hours after it is stopped. When the influence of the kairin is exhausted, which

occurs generally in from two to three hours according to the dose, the temperature rises, and then the patient experiences a feeling of chilliness, and even a severe rigor is far from uncommon. (Chart shown illustrating the effect of kairin in a case of cancer of the ascending colon.)

This medicine has an advantage over quinine in so far as it brings down the temperature more rapidly, but it labours under the serious disadvantage that its effects are more transient.

Antipyrin is a medicine still more recently introduced, and is a synthetic alkaloid belonging to the group of chinilin derivatives, for which we are indebted to Dr Knorr of Erlangen. The dose is from 15 to 30 grains, and the first effect of its administration is a dilatation of the cutaneous vessels, which is soon followed by a fall of temperature, accompanied by sweating. The normal temperature is not influenced by it, but where fever is present three hourly doses of 30 grains generally bring it down to or below the normal. (Chart shown illustrating the effect of antipyrin in a case of typhoid fever.)

Being so recently introduced, my experience of antipyrin is not so great as that of kairin, and I am therefore not prepared to speak dogmatically with regard to it. But while both medicines must be regarded in the light of very certain antipyretics, my present feeling is in favour of antipyrin. It is comparatively devoid of taste, is readily soluble in water (1 in 3 in the cold), so that it may be administered subcutaneously if desired, and it does not discolour the urine. It not only lowers the temperature with at least as much certainty as kairin, but is as a rule more permanent in its effects; the latter too often produces vomiting, headache, and epistaxis, and when the temperature begins to rise a rigor very frequently ensues, which, to say the least, is an unpleasant occurrence, and which may also lead to errors of diagnosis.

Quite recently Mingazzini has tried the effect of giving the two medicines in combination, and his experience is that in this way a more marked and more permanent fall of temperature takes place than when either is given separately; and, further, that when kairin is given along with antipyrin, the former does not give rise to the inconveniences which are apt to ensue when it is given alone. (Chart showing the effect of a combination of kairin and antipyrin in a case of peritonitis.)

Such are the measures which in my experience are capable of fulfilling the object in view—that of combating the element of fever when, from its intensity, or from its long continuance, it threatens the safety of the patient.

The President, in the name of the Society, thanked Prof. M'Call Anderson for his paper, and the trouble he had taken in coming from the West for the purpose of discussing this most interesting subject with them.

Prof. Grainger Stewart said it was a matter of gratification to himself, and he was sure also to every member of the Society, to have had the advantage of hearing Dr M'Call Anderson on such a subject. The Society was much indebted to him for coming to Edinburgh and giving them such an interesting and practical paper. Any one accustomed to teach must have noticed the teaching faculty manifested in the statements laid before them, the clearness with which facts were marshalled, the lucidity with which every statement was made. To begin with the latter part of the paper, it seemed to him that if any of them were in doubt with regard to the powers of the two newest antipyretic drugs, kairin and antipyrin, they had in the tables before them evidence of considerable value to satisfy them on that point. Those of them who had had opportunities of trying these drugs would agree that the German observers had not claimed too much in saying that they had powers equal to quinine as antipyretics. He did not know that in the meantime he would be prepared to say absolutely that he would have the same confidence in them as in quinine for reducing temperature. If he had an anxious case of pneumonia with the pyrexia threatening life he would still give the preference to quinine. At the same time, he thought it quite possible that these new remedies, either singly or in combination, possessed some advantages over quinine, in respect that some of its disagreeable effects might perhaps be avoided. He should like to ask Dr M'Call Anderson if he was in the habit of using salicylate of quinine, a drug introduced by Dr Graham Brown when Resident Physician to the Infirmary. This drug had a remarkable power of diminishing temperature, and while sometimes it seemed to have an unfavourable action on the heart, yet, as a mere depressor of temperature, he would claim for it a high place. Among other things, he thought they were bound to recognise the fact that Dr M'Call Anderson had done good service, by the suggestion he made a number of years ago with regard to the lowering of temperature by the method he had described so clearly that evening. By this plan there could be no doubt that, if carried out as indicated, it was of great service in the way of lowering temperature and diminishing the discomfort felt by patients in the febrile state, while it did not involve the dangers which some of the more complete methods of applying cold were attended with. The first part of the paper opened up a question in which Dr M'Call Anderson had interested them a few years ago when he published his treatise "On the Curability of Acute Tubercular Disease." In the profession generally there had been a good deal of scepticism with regard to his opinions on that subject. It would be interesting to have the opinions of members of the Society elicited. For his own part, he had no doubt that cases of acute catarrhal pneumonia of great severity with high temperature not unfrequently yielded to

treatment. It was a different question with regard to a truly tubercular process. Did they ever get a recovery in such cases? Dr M'Call Anderson claimed that he had seen this take place. There was one fact he could contribute of some importance, that he had seen post-mortem obsolete miliary tubercles over the surface of the peritoneum, pleura, and pericardium, and throughout the substance of the lungs and liver. It was clear that such cases must have passed through attacks of widely diffused tuberculosis without succumbing to it, the death being due to some other disease long after the tubercular action had run its course. This seemed to point to the correctness of Dr M'Call Anderson's view that the unhealthy action might subside, and a practical recovery of health occur.

Mr Joseph Bell, on the call of the President, said that he felt indebted to Prof. M'Call Anderson for his paper, which had given him a good deal of information. There was one thing surgeons were anxious to get, a reliable drug to give prior to certain operations, more particularly some of the more common ones on the urethra, such as the passage of a catheter. There were some urethras that would bear any amount of handling, but there were others that could not be touched without causing febrile disturbance. He did not know how frequently this happened till he began to note the temperature in every one of his cases where this had to be done. He had for some time past been in the habit of preparing his patients every time instruments required to be passed by giving a large dose of quinine shortly before. In most cases the quinine did very well. In others it did not suit at all, and he was glad to hear of such a drug as antipyrin, where the effects were as good and not so lasting. As an example of the way in which some of these cases were affected, he might mention one case, in which the temperature after the passage of an instrument went up on several occasions to over 106° F., and yet all the time did well surgically, no cystitis nor any other mischief occurring, and the stricture being cured.

Dr Byrom Bramwell desired first to express the pleasure with which he had listened to Prof. M'Call Anderson's paper. He agreed with Prof. Stewart that there was some pathological evidence in favour of the view that acute tuberculosis was curable. He had met with more than one case in which what appeared to be tubercle in the pleura and lung substance was cured by fibrosis. He also thought he had seen cases in life in which what appeared to be tubercular meningitis went on to a favourable termination. He was not by any means alone in holding this view. Dr Bristowe, for instance, in a recent number of *Brain* had reported two cases which appeared to be of this nature. With regard to the case which Dr M'Call Anderson had related, he was not sure that he could agree with him in thinking it to be one of acute phthisis. He would like to ask him how he

would distinguish between such a case and one of pneumonia undergoing a slow resolution. He remembered one case in which this difficulty in diagnosis had occurred. A sailor who had been ill for some weeks at sea came under notice with dulness over both apices, and fine metallic crepitations. The case appeared to be one of acute and rapid tuberculosis. Contrary to expectations the patient gradually and slowly mended under the dietetic and antipyretic treatment. He (Dr Bramwell) then came to the conclusion that probably the condition had been one of double pneumonia at the apex, a very rare condition. During the stage of resolution it was practically impossible to distinguish between such a condition and acute phthisis. He did not see anything in the clinical symptoms of Dr M'Call Anderson's case as they were read which appeared to him inconsistent with the view he had just suggested. In Dr M'Call Anderson's previous cases there was evidence of peritonitis, but in this case it was not so. The diarrhœa that occurred could hardly be brought forward as an evidence of tuberculosis. It would be difficult to realize that there could have been any tubercular ulceration of the intestines to have caused it. He had ventured to express this opinion with all deference, as it was difficult to grasp all the particulars of a case on hearing it read for the first time. The general question of fever was a difficult one. Some authorities, and Nothnagel, for instance, deprecated treating of fever. Hysterical rises in temperature were interesting, but Dr M'Call Anderson had not alluded to them. Some of them were doubtless fictitious, but there were others in which the patients were carefully watched, and all sources of error eliminated, and yet the temperature rose to an extraordinary degree without any danger to life. Notwithstanding such cases, they must agree with Dr M'Call Anderson that when the temperature reached a high point it was usually injurious, and must be actively combated. They must also agree with Prof. Grainger Stewart, that credit was due to Prof. M'Call Anderson for introducing such an easy method of reducing the temperature, more particularly where it was due to some localized lesion such as peritonitis. With reference to salicin and salicylate of soda there was one point he should like to mention. Long before Dr Maclagan of Dundee drew attention to the value of these drugs in the treatment of rheumatic fever, Sir James Simpson was in the habit of prescribing "the bitter principle of willows," as he called it, in the treatment of rheumatism. He did not know if Sir James had prescribed it in rheumatic fever. In mentioning this fact, he did not wish in any way even to appear to detract from the great merit due to Dr Maclagan's original discovery.

Mr A. G. Miller corroborated Dr Bramwell's statement regarding Sir James Simpson's recommendation of salicin. He remembered its being used in the Cowgate Dispensary 25 years ago instead of quinine, as it was so much cheaper. Dr M'Call Anderson, in the

earlier part of his paper, speaking of giving food to fever patients, mentioned also the administration of stimulants, by which he supposed alcoholic stimulants were meant. It seemed to him that the two, food and alcohol, should be widely separated. Alcoholics administered in fevers might act in a totally different way from that of giving nourishment. In peritonitis, by administering opium life could be maintained on a much smaller quantity of food. He believed that alcohol acted in much the same way. Another point he wished to mention—the high temperatures in connexion with operations on the urethra mentioned by Mr Bell. He had had several instances of this. In one quite lately, after bougies were passed, the temperature went up to 106°·2. The next morning it was sub-normal. The patient was apparently not one bit the worse. This raised the question, What was fever? Dr M'Call Anderson seemed to have taken for granted that high temperature meant fever. Such cases as these, and the hysterical cases referred to by Dr Bramwell, seemed to indicate that fever was something more; therefore a remedy that simply brought down temperature was not necessarily an antipyretic. With reference to salicylate of quinine, when he had charge of the erysipelas wards, he found that the combination of carbolic acid with quinine enabled him to bring down the temperature. With smaller doses of quinine ℥j. or ij. of carbolic acid to 5 of quinine acted as well as 10 or 20 of quinine alone.

Dr Clouston said many of them would like to hear from Dr M'Call Anderson the probable reason of the fall of temperature in those cases he had mentioned, whether the drugs acted by controlling the diseased nutrition in those localized conditions, or whether they acted by depressing the nervous action. High temperatures were caused in very different ways. High neurotic temperatures were, no doubt, caused by direct excitation of the nervous system. In complicated cases, as mania with the puerperal state, medicines like quinine acted apparently as purely nervine depressants in a direct way on the nervous centres. He had seen quite as marked results in puerperal mania from the use of quinine in doses from 10 to 20 grains, as Dr M'Call Anderson had obtained with the new medicines.

Dr Hunter (Linlithgow) said it might be within the recollection of members of the Society, that he had read a paper before them on an "Epidemic of Typhoid Fever, with special reference to its Treatment by Antipyretics." He then produced charts to show that the treatment by antipyretics resulted in a lower death-rate than had been obtained by him by any other method. The antipyretics used were the salicylates, chiefly the salicylate of soda. Dr M'Call Anderson might recollect reading a paper in the *Lancet* by Dr Tomkins of Manchester, on the treatment of typhoid fever by sodium salicylate, in which he stated that he had found the latter drug a most trustworthy antipyretic. That it did depress

they must allow. A more extended experience had led him to combine with the soda salt four or five grains of quinine. From this combination he had got the same results as from the more expensive salicylate of quinine. With regard to the general question of fever there was one idea that occurred to him during the discussion. Every patient did not bear fever alike. Some had a temperature of 105° or over, and did not seem much put about by it. Others, children especially, in whom convulsions were apt to occur, suffered greatly. Lower the temperature and the sufferings at once disappear. The child goes to sleep, and the mother would say that the draught her child had got must have contained an opiate. In cases in which no grave symptoms appeared there might be no treatment required beyond watching. An objection to quinine was its expense. A cheaper antipyretic was sulphate of cinchonidine, which was very trustworthy. Another valuable antipyretic not alluded to, was a combination of carbolic acid and iodine. He had used this in a case of typhoid, in which the high temperature was probably due to blood-poisoning induced by the intestinal ulceration, and with excellent results.

Dr Foulis knew nothing more interesting than the fact that the temperature of the human body, in a state of health, in all parts of the world, was about 98°·4 F. Should the temperature rise above this, then we had fever. Sometimes the temperature of the body in fever went up to 110° F., and in considering the nature of fever, it should always be borne in mind that before the heat or caloric becomes sensible, *i.e.*, affected the thermometer, an enormous amount of such heat became latent. What did we know of the causes which produced such a high temperature in fever? Almost nothing. He would like to ask Prof. M'Call Anderson how it was that he had not mentioned aconite as a means of preventing and reducing fever. He doubted if there was any physician who had not had satisfactory results from its scientific use. It used to be taught that the thermometer and aconite should go hand in hand. An interesting and most important question was, How did all these fever-reducing drugs act? Did they act all in the same way? The cold cloths acted in abstracting heat or caloric from a body hotter than themselves, but it was more important to know how such drugs as aconite, antipyrin, and kairin acted. There was a method of reducing temperature which had not been mentioned, and that was the application of warm moist poultices to the whole abdominal region. He had employed it in many fevers, but particularly in scarlet fever, and at the same time was in the habit of giving his patients as much cold water to drink as it was possible to get them to swallow. This treatment was carried out day and night in the case of scarlet fever, and not a drug of any kind was given to his patients. Constant diaphoresis was kept up by this means. In the course of ten years he had not lost a case of scarlet fever by this treatment, and he held

the opinion, that it was decidedly wrong to administer aperients and such drugs as determined blood to the mucous membrane, rather than to the skin, in such a disease as scarlet fever. Most fevers had a course to run, and could not be cut short by any drugs; but high temperature could very generally be reduced by free diaphoresis and diuresis. It was a most important matter that they should have a better acquaintance with the physiological action of the drugs they employed. It did not become them, as scientific practitioners of medicine, to use drugs of the action of which they knew little or nothing.

Dr Ireland said that if Dr M'Call Anderson were a prudent man he would ask for another night to answer the great number of questions which were being put to him by so many distinguished physicians. He had another question to add to the number. There was always a difficulty in his mind as to whether intermittent fever should be treated in the hot stage at all. It was a complex fever with a hot, a cold, and a sweating stage. When the cold stage was going on they never thought of depressing the temperature. When they came to the hot stage, in which the distress to the patient was greatest, ought they to try to relieve him by depressing the temperature? He had always felt this a difficulty. It was generally held by those who had great experience, that there was no use in giving quinine during the hot stage. It was given in a large dose at the end of the fever to prevent the next attack. The administration of diaphoretics to make the skin moist appeared to be following the natural course, as the sweating stage gave relief. This was a matter of very great interest so far as hot climates were concerned.

Dr Affleck said that with regard to the cases of alleged acute tuberculosis, he quite agreed with the view taken by Professor M'Call Anderson, and at the time he published his papers, he felt inclined to write to him that he had seen cases in which the diagnosis could hardly have been anything but tuberculosis, and where a cure was effected on the lines recommended by him. With respect to the use of antipyretics in fever—having been constantly in the habit of treating fevers by antipyretics and otherwise—he could say that his experience was that the best of them would sometimes disappoint expectations. The fever they were most available in was, undoubtedly, typhoid—a long continued fever—where it was of importance to restrain the febrile action. There was really danger to the system from the continuance of fever, apart altogether from the poison which may be in operation. He thought that both experimentally, as shown by Dr Wood—by applying heat directly to the body—and also in such an affection as sunstroke, there was evidence that there was danger from mere febrile action, and in a long fever such as typhoid it was of consequence to restrain, as far as possible, the course of the temperature. In many cases the fever was not of great intensity,

and they could cut it short by any means in their power, because there was local action going on; but when it attained a certain height there was great danger of it passing out of their control, and the timely treatment or prevention of it was of importance. He had used most of the antipyretics known, and at the present time was using chiefly antipyrin with very great benefit. It had many of the advantages of quinine without some of its disadvantages. It was more easily administered, and did not sicken, as quinine sometimes did. It had yielded favourable results in lowering temperature and keeping it down. It must be borne in mind that by these antipyretics they did not shorten the case, but still it was of great importance to convert a continued into an intermittent fever, so as to break continuity, and give the body a chance to rehabilitate itself. This afforded the patient a better chance of life. They should interfere before the temperature rose too high, because it might pass to such a height that nothing would reduce it and save life. He had a chart with him of a case in which the temperature rose very rapidly from 105° to 108°·2. Cold baths were not available, but the wet pack was applied. The temperature went down, but rose again when the sheet was removed. It was again applied and the temperature again brought down, but the patient died. When the temperature rose over a certain point the heat-regulating mechanism had passed from under control, and antipyretics might be of no use. With regard to initial high temperatures, he agreed that the use of some hot application might be of benefit in the exanthemata. He had used it frequently in the form of a hot pack in scarlet fever. His experience of that fever had not been so fortunate as that of Dr Foulis, because he had seen cases of malignant scarlet fever where no human means could prevent a fatal result. The pre-agonic temperature (so called by Wunderlich and others) in scarlet fever, alcoholism, and injuries of the head—a temperature which suddenly rushed up before death—was one in which antipyretics would have no effect at all; but the high temperatures in rheumatics and typhoid fever were those in which they should be tried. In addition to the administration of drugs, the topical applications for lowering the temperature and the sucking of ice should be tried. As to the question of dieting, the diet he found most suitable was undoubtedly milk. His experience in the typhoid wards was that the severe cases would not take anything but milk; the milder cases might have a little more appetite, and were sometimes gratified with bread puddings, etc. As the case advanced the employment of soups, custards, and albuminoids was indicated. They continued milk till convalescence, and sometimes even through it. A slight change in diet, even during convalescence, might be followed by a rise in temperature, which might bring on a relapse. It was important to attend to this in scarlet fever, where it was much more difficult to make the patients obey,

because they did not feel in their convalescence so weak as those recovering from some of the other fevers. He had seen severe kidney complications from patients taking animal food surreptitiously during their convalescence. He thought it was of importance to employ the thermometer during convalescence as well as during the fever. He did not know how they would find out relapses sometimes if they did not. The pulse was not always a guide, as it was occasionally where the temperature was high. He should mention that they could hardly dispense with the administration of alcoholic stimulants. There were a few mild cases in which they might not be given, but in most they were a necessity. When the first sound of the heart became very feeble, they could not be dispensed with. He often gave alternately with them the diffusible or ammoniacal stimulants, more particularly in cases where there was a tendency to blood stasis.

Dr Graham Brown rose to make a statment of a historical nature regarding the thermometer. It so happened that certain manuscript books belonging to an Edinburgh physician who died over twenty years ago had recently come into his possession. That gentleman had attended at the Infirmary in 1803, and it appeared from his notes that it was the regular practice in the clinical wards to take the temperature by means of the thermometer.

Dr P. A. Young rose to mention a fact, showing that, as Solomon had it, "there was nothing new under the sun." It had occurred to him on hearing Dr Bramwell's remark about Sir James Simpson's recommendation of salicin. A relative of his, the wife of a medical man at the Cape, was being treated for rheumatism. Among other things an old Hottentot woman recommended her to take a decoction of willow, which apparently had been known to these people as a household remedy for a very long time. He had been much interested in Dr M'Call Anderson's paper on the Curability of Phthisis, and had adopted the treatment he had suggested, but without the same good results. He now saw that he had not fully comprehended the importance Dr Anderson attached to the application of cold to the abdomen, and trusted that by carrying this out with greater fulness, he would be more successful.

Dr Haddon was understood to object to the cold-bath treatment of fevers and the local application of cold, even when done by means of rubber pipes as recommended by Dr Roberts of Manchester, and to enter his protest against the use of such drugs as kairin and antipyrin.

Dr Allan Jamieson had heard with much pleasure the remark that fell from Dr Graham Brown. It was quite true that the thermometer was used in the investigation of disease long before Dr Aitken introduced his self-registering thermometer. It was the fact of the thermometer not being self-registering that prevented its being so extensively used as it was now. The thermometer had then to be read *in situ* and in the case of

infectious diseases, such as typhus fever, the disease was apt to be communicated to the observer. Hence it fell into disrepute.

Dr Hunter (Linlithgow) asked if the bacillus was looked for in the case of phthisis.

Prof. M'Call Anderson said that so many questions had been asked, that if he had a small dose of antipyrin with him he would be inclined to take it before attempting to answer. There was one gentleman—Dr Affleck—with whose remarks he most thoroughly agreed, and he was glad to hear from him of his success with antipyrin. Dr Ireland had asked him about the treatment of the hot stage of intermittent fever. He certainly would not, as a rule, give antipyrin or any other antipyretic, as the patient's life was generally in no danger from the fever. He had, however, found that three drops of nitrite of amyl often stopped a paroxysm of ague. It did not cure, but it was sometimes useful to be in a position to avert a paroxysm. Prof. Stewart had referred to the subject of the curability of phthisis. He had not intended to have spoken of this that evening, but as it had been introduced he might say a word or two. Probably most of them were aware of the fact that a patient might die of a certain disease, and at the post-mortem tubercles be found in the lungs, which had given rise to no symptoms during life. When they gave rise to symptoms, it was by causing inflammation in the lung substance. They recognised two forms of tubercular disease—pneumonic phthisis and miliary tuberculosis. It was now, however, generally conceded that both of these were essentially tubercular in their nature. It was said in the review on his work in the *Edinburgh Medical Journal* that the cases he had published were cases of pneumonic phthisis. He regarded that in the light of a compliment, as acute pneumonic phthisis was in his opinion the more surely fatal of the two forms, because it produced such rapid and wide-spread destruction of the lung substance. Passing on to other questions, Mr Miller had very truly said that mere high temperature did not constitute fever, as was shown by hysterical temperatures, but every one would admit that the elevation of temperature in such a condition as typhoid was an element of the fever, and a valuable one to them, as it could be easily gauged. If they found a patient with a high temperature in a state of delirium, very rapid breathing, and uncountable pulse, and sleepless, and if after giving an antipyretic he fell into a sound and refreshing sleep, and the temperature at the same time came down, then they had a right to say that for the time they had removed or diminished the fever. Dr Foulis had charged him with not entering into the physiological action of the drugs, but his paper was essentially a practical one, and he was not inclined to enter into theories, nor could he agree with Dr Foulis that they should not use remedies the action of which they did not know. If he did, he would have to retire from the profession altogether, as we knew very little of the

action of the majority of the drugs which were used and found efficacious. He had not mentioned aconite, because he did not believe in it as an antipyretic. He did not think any hospital physician did, at least he did not know of anyone who was in the habit of using it in a case of hyperpyrexia. Dr Hunter had spoken of the value of the salicylates. His opinion differed from the experience which he (Prof. Anderson) had had, and there was one thing he would say, and that was, that he did not think they could gauge the action of antipyretics satisfactorily in general practice. They required to have a careful and skilled nurse to look after the case, and take the temperature every hour, as was done in the cases he had brought before them, and whose charts he showed. They should be careful of testing the value of the salicylates, unless they had an hospital in which their observations might be made with the utmost accuracy. He would, however, remember what Dr Hunter had said, and would be glad to give the salicylates another trial. As to salicylate of quinine, he had no experience of it. If it was a valuable antipyretic, he would be inclined to say that it was the quinine, not the salicylic acid, which did the good. Dr Roberts's method of applying cold by rubber piping had been referred to. They had long been familiar with this use of indiarubber tubing in Glasgow. Leiter's metal tubes were even better than the rubber pipes, as they were much less liable to accidental obstruction, but they were heavy, and patients generally much preferred the use of iced cloths employed in the way he had described.

Meeting XI.—June 17, 1885.

Dr H. D. LITTLEJOHN, *President, in the Chair.*

I. EXHIBITION OF PATIENTS.

Dr Allan Jamieson showed two cases of a rare eruption of the skin—LICHEN PLANUS. The first was a woman aged 48, a patient of Dr Alex. Bruce, who had kindly brought her to him. The eruption had lasted only about three weeks. It appeared first on the chest, then spread down the legs and arms. It was most characteristically seen on the chest, and consisted of papules and patches, the papules presenting the appearance usually seen in lichen planus, —dull red spots, glancing on the surface, in this case round and with a slight depression in the centre. It was accompanied with itching. The case was one of those described by Lavergne as acute lichen planus, in which the eruption spreads very quickly over the body. It was not known to Hebra, who spoke of the great chronicity of

lichen planus, and said this was a distinction between it and a syphilide. The second case was that of a gentleman who had very kindly come to allow his eruption to be shown as a foil to that in the other case. It showed a rarer form of the eruption of lichen planus. The papules were very small, minute and angular, not round, arranged in lines, and almost of the normal colour of the skin. The disease began four years ago on the inner surface of the thigh, and remained there till three or four weeks ago, when it began to spread all over the body, except the face, where lichen planus had not yet been seen. The groups of papules were surrounded in this case by a slight erythema, so that at first sight it appeared like an eczema. It was accompanied by itching, and had developed during a course of arsenic which he had been taking since winter, first in small doses, then one-twentieth of a grain, three times a day; showing there were some cases of lichen planus in which arsenic was of no avail.

II. Original Communications.

1. THE ETIOLOGY OF RICKETS.

By J. Macdonald Brown, M.B., F.R.C.S. Ed., F.R.S.E., Lecturer on Anatomy at Surgeons' Hall, and Surgeon to the Western Dispensary, Edinburgh.

Until comparatively recent times nothing definite was known of the disease called rickets, and certainly its etiology was misunderstood. Even in the present day the most conflicting ideas prevail with regard to its causation. It is in the hope that its discussion may clear up some of the confusion of opinions which exists that I venture to occupy your time for a little. With this end in view I shall submit to you the statistical results of an investigation conducted by me during the last four years.

Before recounting the more generally accepted ideas regarding the etiology of rachitis now, it might be interesting to note some of the supposed causes to which in former times rickets was attributed.

As is well known, the disease was recognised as early as 1554, when Theodosius wrote a paper upon it. Before that time it had been vaguely hinted at. A glance at the extensive bibliography of the subject shows us that in the succeeding centuries, at various periods, many writers attempted to limit its symptoms, to define its causes, and to prescribe the treatment. Not only has rickets been the subject of many monographs in the 17th, 18th, and 19th centuries, but at various periods committees have been formed, and discussions have been held in order if possible to clear up the mysteries of the disease.

Like most other well-marked diseases, it was recognised very slowly, and most of its symptoms grouped or classed with those

of other lesions. As medical knowledge, however, became clearer and better defined, it was recognised as a distinct disease. For a long period of time after its symptoms had been fairly well described, the most absurd and conflicting ideas were held with regard to its etiology. We find it attributed to scrofula, scorbutus, arthritis, syphilis, etc. Some authors maintained obstinately that the disease was identical with mollities ossium. Thus we find that in the 17th century Storch defines rickets as a disturbance of the growth of the bones and muscles. We discover later that Hufeland and others designate it as a scrofulosis localized in the osseous system, or perhaps a syphilis so situated.

Many regarded rickets as a mere symptom of various dyscrasiæ, but all concurred in designating it as a local disorder or symptom. I need scarcely say that we now not only consider it a distinct disease, but that it is also well known to be not a local but a general one; that the distortion of bones and resulting deformity are not the main elements or features of the disease, but rather the results of its action.

Before proceeding further, I should like to emphasize this fact, that in rickets we have to deal with a general disease. It is a disease, as Haward[1] states, in which the bone changes are not always present, and with reference to which he who looks only for these signs of the disease will fail to recognise some of the most severe cases.

No doubt in most cases the main stress falls upon the organs of locomotion, viz., the muscles, ligaments, and bones.

Without attempting to enter into an exact description of the symptomatology of the disease, I think it advisable to tabulate the following more prominent symptoms:—

Restlessness and feverishness.

Sweating.

Sensitiveness of all parts of the body to even gentle pressure, existing in muscles as well as bones (Gee).

Gastric and intestinal changes.

Enlargement of the spleen, which is in many cases very marked.

Enlargement of the liver.

Enlargement and induration of lymphatic glands.

Affections of bone, *e.g.*, softening of shafts of long bones, and enlargement of their epiphyses, etc.

Relaxation of ligaments.

Muscles weak, pale, and flabby.

Urine irritating, and often acid.

Dickinson[2] affirms that when the visceral change is most marked, that in the skeleton is seldom extreme—as if the disease spent its force in one direction or the other.

Senator[3] defines rickets as a "disorder of nutrition peculiar to

[1] *Path. Soc. Trans.* 1881. [2] *Ibid.*

[3] Ziemssen's *Cyc. of Med.* vol. xvi.

childhood," essentially found at the time of functional digestive change, from the sixth to the thirtieth month being the usual period. It less frequently occurs up till five years, after that age it is exceptional.

Starting then with the fact that in rickets we have to deal with a constitutional disease, let me very briefly discuss the various theories which at the present time are held regarding its etiology.

The most prevalent of these are—

1*st*. The dietetic theory.

2*nd*. What I might venture to call the hygienic theory.

3*rd*. The diathetic theory.

The first or dietetic theory, as I shall point out, is an extremely beautiful one, but, unfortunately, as far at least as my investigation can be considered of value, it is not absolutely correct.

Numerous experiments upon cats, dogs, fowls, and other animals, made by Guérin, Tripier, Chossat, Milne-Edwards, Friedleben, Heitzmann, and others, have shown that an unsuitable diet given to young growing animals very soon produces marked effects upon the osseous system. As regards the true nature of this change there is much diversity of opinion. Guérin, Heitzmann, and others, believed that true rickets was produced, while others, including Tripier, Chossat, Friedleben, etc., averred that although deformity of the bones and other changes were induced by such treatment, true rickets did not occur.

The true dietetic theory of the etiology of the disease appears to rest on the fact that when lactic acid, or some irritant having similar properties, is administered to animals of suitable age it induces true rickets.

Lactic acid acts on bone in two ways—1*st*, It causes an increased formation of actively growing bone, *e.g.*, epiphyses, etc.; and 2*nd*, it dissolves the earthy constituents of formed bone, such as occurs in the shaft of a long bone.

Wegner produced true rickets by the administration of small doses of phosphorus, lime being withdrawn from the food. Heitzmann, finding lactic acid in the urine of rickety animals, fed animals with this acid, and also administered it to them hypodermically. Animals so treated exhibited gastric and intestinal disturbance, and after a few months displayed bony deformity, which was of course mainly due to mechanical causes.

The theory as defined by Senator is, "that some irritant matter endowed with such properties as phosphorus is either introduced into the body with the unsuitable food, or else generated from it in the system (the latter being the more plausible hypothesis). "Further," he says, "we must suppose that the food either is deficient in lime, or that the absorption of lime salts from the alimentary canal is in some way hindered."

Let us take the case of a child which during the first few months of life has been fed upon suitable milk. Suppose this child be

then put upon unsuitable diet, such as potatoes, farinaceæ, or even meat; as expressed among the poorer classes of Scotland, "it gets a bit of what's going." Lactic fermentation is induced in the stomach, and lactic acid formed, some of which is absorbed into the blood. Gastritis and irritation of the alimentary canal follow, probably with the further production and absorption of lactic acid. The lactic acid, carried by the circulation to the bones, acts upon them in the double way already mentioned, with the result that the growing parts are increased in size, while a large amount of the earthy constituents of the formed bone becomes dissolved and removed, or possibly is even prevented from being deposited. The earthy phosphates are believed to be excreted in the urine and fæces. Klecinsky has shown that they are found in the latter. Marchand, Lehmann, and others have detected lactic acid itself in rickety urine.

It is, of course, possible that other osteoplastic irritants besides phosphorus and lactic acid may exist. Perhaps, as Senator points out, that just as the spleen in certain hyperplastic states generates acid in excess, so a similar formation of acid may take place in growing bone tissue (a tissue which resembles the spleen in many respects), of acid capable of dissolving the earthy salts, and formed in such abundance as actually to prevent the calcification of the new tissue.

A strict interpretation of this theory applied to the disease would lead us to believe that rickets would, in most cases at least, be induced by the removal of a child from simple milk food, as supplied by the mother's breast, or by suitable artificial means, before such time as its salivary glands permitted it to be capable of the digestion of starch, and its being placed upon a farinaceous diet —in short, too early weaning either from the bottle or from the breast. There is, however, a wider definition of the dietetic theory, which is now held by most of the observers who incline in this direction. They state that improper feeding is the cause of rickets, but believe that while too early weaning is a fruitful cause, that prolonged lactation, on the other hand, is likewise productive. The quality of the mother's milk must also naturally be taken into consideration.

Senator states that it is not the particular food, or any special quality or constituent of it that causes rickets, but that any diet unsuited to a child's age, and the state of its development, will bring on rickets. In many cases rachitic children have been the victims of over-feeding. As regards the deficient quantity of earthy matter which must co-exist with the presence of lactic acid in order to effect the production of the disease, we must remember that an abnormal quantity of salts is removed from the body by diarrhœa, etc., and, doubtless, among these the calcareous compounds taken in with the food. As Hilton Fagge,[1] however,

[1] *Path. Soc. Trans.* 1881.

has stated, "the weak point about Senator's speculation is that no excessive formation of lactic acid has been proved to occur."

Morgan,[1] in 1882, examined 150 cases in ordinary hospital routine. He found the average time of weaning to be 10½ months, and that the immense majority occurred among poor people. He believes that his statistics as regards weaning are unsatisfactory, because in most cases artificial food is given to supplement the ordinary milk before actual weaning has taken place. He lays great stress upon the fact, that in almost all the cases which came under his observation there was some bowel trouble present, and believes that this neglect of the condition of the digestive organs is far more productive of rickets than the mere quality or nature of the food, more especially as many cases of rickets occur which cannot be ascribed to the fact of such a diet as a starch one.

The second, or what I have ventured to call the hygienic theory, affords a much larger area for speculation. It is more generally accepted than the dietetic hypothesis. Its supporters are inclined to believe that the disease is due to any cause which impairs or lessens the general well-being of the child during the early months of life, such as bad air or surroundings, want of light, improper feeding, and all the other ills of poverty.

Shaw[2] states that the disease occurs among the poor, chiefly the very poor; that it is mainly found in the alleys and slums of our great cities; that it is more marked and more common in this country than on the Continent or in America, because in these parts the system of living affords better hygiene and food. He indeed sums up as follows:—"Rickets is scarcely ever found except among the children of the poor who are miserably fed, badly clothed, and who live in the narrow streets of large cities in a confined and unhealthy atmosphere."

Holmes[3] believes the disease to be due to a varying combination of all the elements of malnutrition.

Sir Wm. Jenner,[4] in his exhaustive monograph on the subject, says—"Among the children of the poor in London, the most widely spread diathesis is rickets. It is, however, by no means limited to the poor, or to London, or even to large cities. I have very often seen it in the children of the wealthy, and even in the children of the wealthy living in the country." He states that "it is essentially and purely a disease of nutrition, not of one part, but of the whole body, and is something more than a mere deficiency of the earthy salts of bone." While disinclined to favour the theory that rickets is hereditary, he affirms that an unhealthy mother has a decided influence on the development of rickets in a child, even although the hygiene is most favourable. He accounts for the fact that the later children of a woman may become rickety on two grounds—1*st*, That a man's wages does not increase in the same ratio as his

[1] *Med. Times*, June 1882.
[2] *Med. Chir. Trans.*, vol. xvi.
[3] *System of Surgery*, vol. iii.
[4] *Med. Times and Gazette*, 1860.

family; and, *2nd*, on account of the impairment of the mother's constitution. He believes that "whatever external conditions are favourable to the formation of hydræmic blood in a child seem also to be favourable to the development of rickets"—impure air, food insufficient in quantity or defective in quality taken daily, deficient light, and want of cleanliness, in short, that whatever elements interfere with nutrition, and so with the formation of good blood, tend to rickets.

Summing up these various opinions, the general belief as regards the etiology of rachitis appears to be that it is due, as Holmes puts it, to a varying combination of all the elements of malnutrition.

Before entering upon the discussion of the third or diathetic theory of the causation of the disease, I wish in a few words to read to the Society the outlines of a short statistical account of the results of my observations, and to suggest the deductions which may be drawn therefrom, as bearing upon the two theories already described.

During the past four years I have examined a large number of children under the age of three years, rickety and otherwise. From this number I collected 100 typical cases of children affected with rickets, and I also went carefully into the history, etc., of a similar number of children who were not so affected. Whenever a rickety child came under my observation in ordinary hospital routine, or otherwise, I endeavoured to ascertain particulars regarding (1), its dietetic history; (2), its surroundings; and (3), its family history. Bearing in mind Morgan's belief that weaning is with most mothers, among the poorer classes at least, a mere relative term, I most carefully investigated the period of suckling, nature of food, history of vomiting, purging, etc. The inquiry necessary to enable me to form an opinion on the second point, viz., the surroundings or home of the child, was even a more difficult matter. I, however, spared no pains in order to possess exact data, and accordingly in nearly every case visited the neighbourhood and dwelling of the child, and made a careful inspection of the sanitary conditions, etc.

The results are as follows—of the 100 cases of rickets:—

I. *As regards Diet.*—71 had been brought up systematically on milk for over nine months, and only in an exceedingly small proportion of these had it been supplemented with any farinaceous matter. I have placed the latter under this heading, because the amount of starchy matter was so infinitesmal as to be scarcely worth noting. 20 children had milk diet largely supplemented with arrowroot, breadsap, etc. 9 cases only occurred in which over-lactation had been carried to any extent.

II. *As regards Hygiene and Surroundings.*—68 children had fairly comfortable homes, in which the light and air were good; 21 lived under circumstances which could not be described as

favourable; 11 lived amid surroundings of squalor and dirt, and hygiene of the most wretched description.

III. *Family History.*—As above there was a scrofulous tendency in about 20, and in about a similar number of cases a history of syphilis in the parents was made out. In nearly 30 cases the mother was weakly before or during her pregnancy. I endeavoured to discover a hereditary tendency in these 100 cases, but my results in that direction did not lead me to any satisfactory conclusion.

Of the 100 non-rickety children, whose condition I investigated in a similar way, over 40 were weakly and ill-nourished, the remaining 60 were fairly healthy.

Of the 40 *weaklings,* three-fourths had during the early months of life farinacea along with or substituted for milk. The hygiene and surroundings of more than half of these were questionable or bad. More than one-fourth were of strumous habit, and six had a syphilitic taint. In the majority of these 40 cases, the mother had some story, more or less reliable, to tell of weakness or illness before or during utero-gestation.

Of the 60 *healthy children,* one-half had been brought up on milk with farinaceous matter, the milk in some instances being almost dispensed with. In 22 cases the hygiene, etc., was bad.

These statistics, gentlemen, may be open to question on the grounds that the cases were exceptional and possibly selected; but I have simply placed before you the results of an investigation carefully conducted in ordinary hospital and in general routine. The fact that a large proportion of the rickety children had (during the early months of life) milk diet appears to me to militate strongly against the dietetic theory of the etiology of rachitis,—while the hygienic hypothesis seems to be largely negatived by the fact that in nearly 70 per cent. of the same class of cases the hygiene and surroundings of the children were good. It must be confessed that the results of these investigations are not altogether satisfactory, inasmuch as that while they are negative of the dietetic and hygienic theories, they do not furnish a more positive hypothesis to replace them. Judging from these statistics, it, however, seems possible to me that a rickety constitution may exist, the effects of which can only be manifested during the earliest years of child life. If such exist, it is exceedingly widespread, and is not specially limited to any country, or to any particular district of a country.

Rickets is by no means confined to this country, as its old title of "English disease" would imply. Senator states that it "occurs all over the globe." It is widely distributed over Europe,—being found in Germany, Holland, Russia, etc.,—and in America also the disease is very well known. As regards its prevalence in the other continents, we have no positive information. Professor Macleod of Calcutta informs me that among the natives of India the disease is unknown.

Some years ago Dr Merei endeavoured to ascertain the comparative prevalence of rickets in different parts of the British Islands; but his results are considered by Jenner as not altogether trustworthy. I thought it might be worth while repeating his experiment, but the result was unsatisfactory. I, however, clearly made out that the disease is not confined to large or crowded cities, but that it prevails in the most healthy parts of rural districts. It is a fact emphasized by Sir William Jenner, and known from wide experience, that rickets is frequently met with in the children of the wealthy, where the dietary and surroundings are unquestionable.

If we accept the constitutional theory, and believe that the rickety diathesis is latent in a large proportion of the population, that it is only capable of manifesting itself during the earliest years of childhood, then all that is required is an exciting cause or causes.

Let us take an example. Suppose two children of a few months old, in apparently equally good health. To each injudicious diet is administered, or the hygiene, etc., in which they are placed is unfavourable, or both. One child develops symptoms of rachitis, the other becomes simply weakly and ill-nourished, but presents none of the features of the disease. Such, to my mind, can only be explained by assuming that in the one case a rickety tendency previously existed, while in the other no such constitution was present. That rickets occurs less frequently among the children of the better classes seems explicable in the same way, that although the constitution may frequently be present, still there is less likelihood of the occurrence of the exciting causes necessary for the production of the symptoms of the malady.

Not infrequently among the children of one family, one or more children develop the disease, although the other children remain unaffected. In such a case we must again assume that the constitution existed in all the children, but that some unnoticed exciting cause or causes developed the disease in some of them, *e.g.*, some illness of the mother during utero-gestation, less careful nursing, some temporary defect of drainage, etc., etc.

To sum up, therefore, why should the existence of a rickety constitution be impossible or unlikely? If it do exist, then the so-called fundamental or original causes of the disease—improper diet, bad hygiene, syphilis, etc.—must be regarded as mere exciting ones. It seems to me that if we accept the hypothesis that the constitution does exist, that we can reconcile many of the existing causes, by believing that in children possessed of the diathesis, anything which tends to lower their health or well-being, be it improper diet, poor hygiene, unhealthy parents, syphilis, etc., will undoubtedly bring about a manifestation of the symptoms more or less severe.

At the International Medical Congress of 1881 one of the chief

features of interest in the Surgical Section was a discussion on rickets, when it will be remembered that Parrôt endeavoured to show that syphilis was the great cause of rickets. So far as I have been able to ascertain, most authorities throw great doubt on the existence of so-called fœtal rickets, and we know that the congenital cases are few. The consensus of medical opinion is utterly opposed to Parrôt's belief; but although syphilis may be discarded as an original cause of rickets, it may certainly be retained as an exciting cause in many cases.

Nearly four years ago rickets was the subject of an important discussion at the Pathological Society of London,[1] and among other things its etiology came in for a large share of it. During it, Sir William Jenner said that he saw no reason to regard rickets as other than a diathesis, by the latter meaning, he said, a certain predisposition to disease. Hutchinson affirmed that "if we are to understand by diathesis a condition of defective health, giving, during a long period, proclivity to certain forms of disease, then rickets is undoubtedly a diathesis; if, on the other hand, the term is restricted to the results of a specific poison—*it is not.*" He classes rickets along with the scorbutic, tubercular, gouty, and leprous diatheses, and still further inclines to the belief that it might be classed with scurvy, gout, etc., as a "diet or assimilation diathesis." "It is the organs of assimilation which are inherited." There is nothing in his theory, he affirms, contradictory to heredity. "We may safely believe" (he says) "that there is nothing improbable in the idea of a hereditary transmission of a constitutional tendency to rickets—that is, to speak definitely, of a tendency to defective nutrition on the part of the organs of assimilation, which may, either with or without the help of contributing causes, at some time bring about such a degree of the composition of the blood as would interfere with the nutrition of the bones." "That alone and quite unaided by them defective food has over and over again produced rickets, therefore defective food is probably the main cause. If rickets be a disease due to defective food, and the mother's blood be in a defective condition, it may easily come about that, although the fœtus originally inherited nothing, it may become rickety during its pre-natal period of life, the mother being not only the progenitor but the nurse during intrauterine life."

Lucas states that rickets is a "purely artificial disease produced at pleasure, and cured with certainty," but believes that it might become hereditary.

Hilton Fagge says, "it is clear that if rickets can be set up by feeding a child improperly, or by any other unfavourable conditions of life, it is not in the strict sense a diathesis."

Goodhart believes rickets to be the expression of improper development rather than of disease.

[1] *Path. Soc. Trans.*, 1881.

Might it not be permissible, gentlemen, to go a step further into the region of theory than Mr Hutchinson has done, and to suggest that in rickets we have to deal with a diathesis in a stricter sense of the word than that in which he employs it. I am inclined to think that the diathesis is, as regards heredity, a general one, and not merely, as Hutchinson states, an inheritance of the organs of assimilation only.

The distinction between the two theories may at first sight seem a subtle one, but I trust it may be clearly understood. It would go largely to confirm and support this theory were there more reliable data to be had on the subject of its actual heredity, but, unfortunately, as previously stated, I could not satisfactorily collect such.

Vogel has seen many families in which the parents showing distinct signs of having been formerly affected, the children have all become rachitic in turn, in spite of every precaution. Rittershain has seen distinct heredity in many cases.

It may very properly be urged that if such a wide-spread disease as rickets be the active expression of a constitutional taint, then evidences of its heredity should be most easily found. This, experience shows, however, is not the case, nor, indeed, can we expect it to be otherwise. The diathesis, as I take it, is essentially a latent one, only capable of active manifestation during the earliest years of child-life, and requiring for such manifestation an exciting cause or causes. Should these latter not be forthcoming, the period of rachitic activity is passed over, and in after life cannot again from the nature of things appear. The man or woman, however, in such a case, although presenting no external evidences of the former occurrence of the disease, still, to my mind, retains the rachitic taint, and transmits the tendency or diathesis to their offspring, which, in turn, may or may not be called upon to manifest its active symptoms. And so generations may pass onward, with only here and there a rachitic child, in whose early life the needed stimulus has been present.

The essential factor in the production of rickets may be, then, I would venture to suggest, a constitutional taint, in virtue of which, when an exciting cause or causes are brought to bear upon the child, its characteristic symptomatology is developed. As Parker[1] says, "if we bear in mind the large numbers of children who are rickety, and that, when grown up, they intermarry and have children, it is no longer easy to disregard hereditary influence as a powerful factor in the production of rickets, and one which every year will become more obvious."

It seems as if in child-life we had to deal with a condition of well-being and good nutrition, any sufficient deviation from which will bring about ill-health in the widest sense of the term, and so render the child peculiarly liable to manifest any constitutional predispositions it may innately or inherently possess. Should

[1] *Path. Soc. Trans.*, 1881.

none such exist, then the child suffers simply from general weakliness and malnutrition, many examples of which my results sufficiently clearly showed, and which experience teaches us are far from being uncommon. If, on the other hand, a predisposition or constitutional taint be present in the economy of the infant, then let the balance necessary for its neutralisation or non-appearance be disturbed by some exciting cause or causes, and under such favouring circumstances symptoms of the diathesis will, as a rule, speedily manifest themselves. Such exciting causes are undoubtedly in many cases improper dieting, poor hygiene, syphilis, etc. It is clear that in a majority of my 100 cases I did not discover distinct exciting causes such as above mentioned, but it is probable that exciting causes did exist, which possibly, from their indefinite or trivial character, I failed to recognise.

This hypothesis, gentlemen, may, I believe, account for the occurrence or non-occurrence of the diathesis at present under consideration. Let the constitutional theory be abandoned, then some other theory must be advanced which will account for the existence of the disease in so many of the cases to which I have referred, where the ordinary ascribed causes, such as improper dieting, etc., were not present.

Theory is all very good in its way, but unless it leads to some practical results it is of little value. To obtain some data which might be of practical use in treatment, seeing that from the study of rickets *per se* so little information can be obtained, the question naturally arises, Can such be procured by considering it in connexion with diseases with which it is supposed to be more or less generally allied? Of such diseases rheumatism, chorea, and various nervous lesions might be instanced. Take rheumatism, for example; do we not find that there is a considerable resemblance between it and rachitis? I think a glance at the symptoms of rickets will convince us that such exists. In both there are — Restlessness and feverishness; profuse sweating; tenderness to touch, etc.; gastro-intestinal changes; affections of bones, ligaments, and muscles; acidity of urine, etc. Salicin and its derivatives have proved of inestimable value in the treatment of the one, why should their use not be of service in the treatment of the other? I understand that they have been employed from analogy in chorea by Brackenridge and Greenfield, and, I believe, with beneficial results. Might not their employment be at least tried in suitable cases of the more severe forms of rickets? I merely throw out this suggestion in the hope that some of the members of the Society, whose experience in the treatment of rachitis is great, may favour us with the maturity of their opinions on the subject.

I must apologise for having occupied so much of the valuable time of the Society to-night, but I am hopeful that it will not have been unprofitably spent if this discussion lead to further

investigation on the subject, which will assist in clearing up the uncertainty which at present exists, and so place the etiology of rickets on a better defined and more accurate basis.

Dr James Carmichael, in response to a call from the President, said he had hoped that Mr Brown would have thrown some new light on the *questio vexata* of the etiology of rachitis. The paper, he thought, fell short of this, although it was an excellent *résumé* of the chief points connected with our present knowledge of the subject. His own personal experience of this disease did not yet warrant him in coming to any definite conclusion regarding its etiology. The morbid anatomy of disease they had a clear enough conception of, but when they came to inquire into the etiological factors and intimate pathological starting point of the morbid processes, they were still, he thought, very much in the dark. In hospital and dispensary, as well as private practice, he had considerable experience of this disease, and among a large number of cases he had noted very carefully over three hundred, in which he had inquired particularly into the dietetic and hygienic conditions. So far as his observations went, experience pointed distinctly in the direction of unfavourable hygienic and dietetic conditions existing in by far the larger proportion of the cases. He admitted that in some, notably those cases met with in the better classes of society, the children were *apparently* under favourable circumstances, and it was not, he admitted, always so easy to explain their etiology on the assumption of anti-hygienic and unfavourable dietetic conditions. Yet the fact remained, that in the great majority such did obtain. He was somewhat at a loss to account for the fact of so large a number of Mr Brown's cases being under what he called favourable conditions in regard to diet and surroundings. He considered that certain fallacies might creep in with reference to the question of what constituted good feeding. In regard to the matter of lactation, he had been in his earlier experience often led astray. In answer to inquiry from the mother as to the nursing of her infant, the child might be getting breast milk and *apparently* satisfied, and yet be insufficiently nourished. In his experience among the poorer classes he found this to be frequently the case, and that artificially reared children were often much better nourished than breast babies. In this connexion he had found it necessary, before arriving at any definite conclusion, to examine the mother's milk, and had been astonished to find this of inferior quality, although perhaps not deficient in quantity, in a large proportion of cases. He was inclined to think, therefore, that defective *quality* of milk in the mother might be a potent cause in the production of the disease, not only from the insufficient amount of nourishment which the child obtained, but from indigestion set up by irregular suckling, which was one of the commonest causes of stomach derangement in breast

babies. Then, among the working classes, the hygienic conditions were generally unfavourable—overcrowding, bad ventilation, and deficient sunlight, were all potent factors in interfering with healthy nutrition and development. The question of hereditary tendency was an interesting one. No doubt some families seemed specially predisposed to the disease, even through generations, but he did not think any special diathesis had been clearly made out. As to scrofula, he had always considered it a curious fact, and one which to a certain extent favoured a diathetic theory, that the two dyscrasiæ were seldom met with in the same family. The theory of the universal association of rickets with syphilis, so ably advanced by M. Parrôt, he thought did not now obtain much support, in fact, was generally disproved. Syphilitic children undoubtedly sometimes suffered from rickets, and he did not doubt that the pre-existence of the one disease might favour the evolution of the other, but he could not go further than this. The various chemical theories of the production of rickets were interesting, but none of them, he thought, could be accepted without reservation. It was generally believed, and with good reason, that there were faults both in the primary and secondary digestion, but when we came to more special inquiry as to the intimate nature of the changes, here our difficulties began. Seeman has pointed out that there is undoubtedly a deficient absorption of the lime salts during digestion, and this he attributes to the hydrochloric acid being diminished in quantity during digestion. The old idea that the lime salts were carried off in excess by the urine is now known to be incorrect, and Petersen and Baginsky, and others, have now demonstrated that there is almost invariably a large excess of these ingredients in the fæces. Roloff and Seeman believe that the deficient supply of lime salts to the bones directly favours the cartilage proliferation, and that the excessive action going on in the osteo-plastic tissue is directly due to this, and not to a special irritant as Wegner supposed. Then, later observers seem rather to view with disfavour the lactic acid theory so long held by Heitzman and others. With regard to the analogy drawn by Mr Brown between rheumatic fever and rickets on the strength of this theory, he thought the two diseases could only be said to resemble one another in so far that there was a disposition to undue formation of lactic acid in both of them. Clinically the analogy completely failed, inasmuch as very different groups of tissues were affected in the two diseases. The blood in rickets had not been shown to present any material change, except occasionally a certain degree of anæmia, apparently due to some concomitant such as diarrhœa. In acute rheumatism the blood changes, on the other hand, were constant, and the whole course and clinical features of the disease entirely different.

Dr Felkin said that in the forty negro tribes he had visited in Central Africa he had never seen a case of rickets in negroes.

The only two cases that had come under his notice there occurred in Arab children. He wished to call attention to the fact that in the vast majority of cases the children of these negro tribes are suckled for two years, and it therefore seemed to him unlikely that over lactation of itself could have much to do with the causation of the disease. He wished it, however, to be understood that during this prolonged period of lactation the women led active lives and remained separate from their husbands. In El Obeid and in Khartoum he had seen a good many cases of rickets in the children of Arab fathers and negro mothers who did not observe the custom of separation. The women in these places were compelled to perform very hard manual labour, and at the same time often became pregnant before weaning their children. The same remarks applied to Arabs' concubines on the east coast of Africa, whose children often suffered from rickets. Most of these children died before attaining the age of three years. Dr Felkin was inclined to think that rickets might be induced, or at least highly favoured by women who were pregnant or greatly debilitated continuing to suckle their children.

Dr James wished to make one remark, that whilst he recognised from the data Mr Macdonald Brown had, he was perhaps justified in drawing his conclusion that to a diathesis the causation of rickets was to be mainly ascribed, he could not help thinking that this was a little unscientific. He said so for this reason. It was a maxim in science that in the causes now at work are to be identified the causes which, similarly at work in previous years, have produced the state of things now existing. That is to say, supposing rickets was a diathesis and certain conditions brought about its manifestation in the individual, these same conditions must have brought about the diathesis. Except it was looked upon as a provisional hypothesis, it was rather dangerous to bring it forward as a permanent conclusion.

Mr Macdonald Brown, in closing the discussion, said that he was unwilling further to occupy the time of the Society, especially as so much business remained to be gone through. Dr Carmichael had taken him to task for having neglected the pathology of the subject in his paper. He (Dr Carmichael) had also mentioned that his statistics as regards rickets differed from those given that evening. In reply to him, Mr Brown emphasized the fact that his conclusions were entirely based upon his own statistics, and that with regard to the pathology, he had studiously avoided it as being foreign to the subject of the paper. In answer to Dr James, he had to say that the conclusions made in the paper were at best provisional, and had been advanced in the hopes that any discussion which they evoked might largely clear up the subject, and so place the etiology of rickets on a surer and firmer basis.

2. THAT THE LATERAL MOVEMENT OF THE ULNA IN PRONATION AND SUPINATION TAKES PLACE AT THE SHOULDER-JOINT.

By CHARLES W. CATHCART, M.B., F.R.C.S. Ed., Assistant Surgeon, Royal Infirmary, Edinburgh; Lecturer on Surgery, School of Medicine, Edinburgh.

SINCE the title of my paper assumes that the ulna moves in pronation and supination, and since this has long been a disputed point, it may be well to recapitulate briefly the arguments on which the present belief is based.

1. *Observation* of the styloid process of the ulna while the hand is being pronated and supinated. If we supinate the forearm, place a finger on the styloid process of the ulna, and then pronate in the ordinary way, it will be quite evident that the bony point moves from its place. It will return again when the hand is again supinated.

2. *Lecomte's Ring Experiment.*—If the radius alone moves, then, since the axis of movement at the wrist passes through the head of the ulna,[1] the transverse diameter of the forearm there will be only about half of the diameter of the circle, part of whose circumference is traversed by the styloid process of the radius in pronation and supination. In other words, so long as the ulna is stationary, it will be impossible to rotate the wrist within a ring whose diameter is only slightly greater than that of the wrist itself. If, on the other hand, the ulna be moving in the reverse direction to that taken by the radius, the real axis of movement will be thrown more into the centre of the forearm, and the styloid process of the radius will traverse part of the circumference of a much smaller circle than it would otherwise do. Lecomte[2] adapted a circular metal ring to his forearm near the wrist, and found that he could easily pronate and supinate his forearm within the ring, when it was being held quite steady. From this he argued that the ulna must also move, and this conclusive proof has since been known as Lecomte's ring experiment. With a little practice, it is quite possible to perform a sort of flapping movement of the hand, in which the ulna is nearly stationary, but it will be then found that the forearm cannot rotate within one of Lecomte's rings, and it will be necessary either to allow the ring to move with the forearm, or if the ring be still fixed, to return to the ordinary rotatory movements of pronation and supination.

It will easily be seen that Ward's axis of movement is not in any way disputed, but only that this axis itself is held to move. In this way the axis of actual movement passes more nearly through the centre of the forearm.

[1] Ward, *Osteology*, 1875, p. 221.
[2] Lecomte, *Archives Général de Médecine*, Aout 1874.

3. *Axis of Movement Arguments.*—If the axis of movement, as laid down by Ward, be the only one, then the centre of movement at the finger should be round the little or ring finger. As a matter of every-day experience, however, this is nearer the radial than the ulnar side of the hand, which can be easily accounted for when we allow that the ulna moves in such a way as to throw the actual axis of motion towards the centre of the forearm, as has been already indicated.

4. *Heiberg's Graphic Method of Demonstration.*—Professor Heiberg[1] took the arm of a subject from the dissecting-room, nailed the humerus to a board, placed a circular ring round the lower end of the bones of the forearm, cut away the hand, and fastened a paint brush into the ends of the radius and ulna, so as to register their movements on a piece of paper held looking towards them. He then moved the bones in pronation and supination, and found that the ulnar brush registered a circular movement in the reverse direction to that taken by the radial brush. This experiment was confirmed by strapping over the ulna in a living arm a steel rod bearing a brush, which, when the arm was bent, projected as far behind the elbow-joint as the styloid process of the ulna was in front of it. A circular ring was adapted at the wrist, and movements of pronation and supination voluntarily performed, when it was found that the ulnar brush performed movements essentially like those registered in the first experiment.

Next as to the kind of movements performed, careful observation of the ulna in the living arm will show that the variety of movement must be that known as circumduction, *i.e.*, a circular movement of the end of the ulna, the result of the combination of movements in two planes at right angles to one another. When the upper arm hangs by the side, and the forearm is flexed to a right angle, one of these planes would be a vertical one produced by flexion and extension movements at the elbow. The other would be a horizontal one, involving a lateral movement of the ulna, the exact seat of which it is the purpose of this paper to discuss.

Possibly this horizontal movement might be incapable of being separated from its vertical combination, *i.e.*, an oblique movement might be alone possible at the elbow. Possibly a horizontal movement might be possible at the elbow; or possibly the movement might not be at the elbow at all, but at the shoulder-joint. In this case either a lateral rotation of the humerus, the ulna being set at an angle to it, or an adduction or abduction of the humerus combined with flexion or extension and rotation, would produce the necessary lateral alteration in the position of the ulna.

The view which I formerly maintained and still hold to is that while this lateral movement of the ulna may, and probably does take place, to a limited extent, by an oblique movement at the elbow, it chiefly occurs at the shoulder-joint. It is to the latter of

[1] Heiberg, *Christiania Videns. Forh.*, 1883, No. 8.

these movements that I wish to direct attention in this paper. No one, of course, denies that in forcible pronation and supination the upper arm moves to and from the side. It is in gentle movements of rotation, however, when the upper arm can be maintained to all appearance steady, that there is difficulty in deciding, and it is in such movements that I hope to prove that a rotation of the humerus is the chief agent for the lateral movement of the ulna. By referring to Professor Heiberg's[1] collection of the literature of the subject, I find that this view is held by Dr R. W. Einthoven.[2] Dr Einthoven believes that by firmly pressing over the condyles with the fingers while the forearm is rotating within one of Lecomte's rings, the slight rotatory movements of the humerus can be recognised, and their similarity noted to purely rotatory movements without accompanying pronation or supination. Professor Heibert criticises this view in an appendix to his paper, and brings two objections against it. (1.) That he has failed to get the same results in repeating Dr Einthoven's experiments; (2.) That by experimenting on a dead arm he has found that as much movement as is necessary is possible at the elbow without calling into play the shoulder-joint at all.

In regard to the first objection, I can only say that, working independently of Dr Einthoven, I had come to much the same results from similar experiments, with only this exception, that I could not say that the ordinary movement of rotation of the humerus was exactly like that in pronation and supination. This, however, I accounted for by the complication of surrounding muscular contractions in the latter case, which was not present in the former. As to the second objection, it might be transposed so as to tell as much *against* the elbow movement as for it; since by arranging a dead arm properly the necessary lateral movement of the ulna might be made to take place at the shoulder-joint without calling into play the elbow. I would rather, however, not appeal to the dead arm at all until we have exhausted every possible means of inquiry on the living. Undoubtedly the inquiry is a difficult one. The amount of movement of the condyles of the humerus is at most very small (about ¼ inch or 5 mm.); the skin is so movable that such slight movements of the bones are difficult to observe accurately, and the contraction of muscles in the neighbourhood is decidedly confusing to one's sense of touch. Is there no other way, one naturally asks, to bring the matter to a more searching test? One way would be to screw long needles or pointers into one or both condyles of a living arm, but this seems to be attended with rather too much risk to be lightly undertaken. On thinking the matter over, however, it occurred to me that there was still another method available, *i.e.*, to study the movements of pronation and supination in patients where by an anchylosis of

[1] Heiberg, *Christiania Videns. Fork.*, 1885, No. 11.
[2] Einthoven, *Archives Néerlandaises*, tome xvii. p. 289.

the shoulder the movements of that joint are eliminated. If the shoulder is not involved in pronation and supination, its anchylosis should make no difference to these movements. If the shoulder, however, is normally involved, then its anchylosis should bring about some deviation from the normal mode of performing pronation and supination, which ought to be traceable to this cause. Such cases of anchylosis of the shoulder, although not uncommon, are not always easy to find when one wants them. I have had an opportunity of examining three, although it is only to the last of them—at present within reach in Edinburgh—that I wish to refer at present. They all corroborate the view that the shoulder-joint is concerned in pronation and supination.

The present case brought before the Society is that of a young man æt. 23. At 8 or 9 years of age he suffered from what appears to have been disease of his right shoulder-joint, for which the actual cautery was freely applied by the late Professor Spence. The disease subsided, but left behind it a firm anchylosis of the joint, which has persisted ever since. As is usual in such cases, the compensatory mobility of the scapula is so great that it would be difficult to believe that his right shoulder had not its full use, were it not that a careful examination manifests the contrary. As a proof of the usefulness of his right arm, I may mention that he served in the militia during part of six consecutive years without the condition of his right arm having been discovered.

When stripped, his chest and back muscles are seen to be well developed, the pectoralis major being rather stronger on the left side, and the upper part of the trapezius on the right. The right deltoid is much atrophied. A large scar resulting from the cautery is seen in front and behind his right shoulder-joint. The right arm from the acromion process to the tip of the middle finger measures 4 inches less than the left. Of this 3½ inches is due to shortness of the humerus, and one-half to that of the forearm and hand. The muscular development of the right arm is good, the deltoid being excepted—upper arm 8¾ inches, forearm 9 inches. On the left side these measurements are ¾ inch greater in each case. The remaining joints of the right arm are perfectly free, and their movements, to all appearance, quite natural.

1. When the right forearm is flexed to about a right angle, and a circular metal ring adapted closely round the wrist and held steady, the patient can rotate his forearm within the ring. With each movement of supination, however, the humerus is brought towards the side to about the same extent as the ulna passes from without inwards in the horizontal part of its circumduction. In pronation these movements are reversed.

The explanation of the inward movement of the whole humerus appears to be that, as a rotation of the humerus is necessarily difficult or impossible, the next best way to gain the same end is to move the ulna laterally together with the humerus. When

this was hindered, so was the movement of pronation and supination.

2. If we still keep on the ring, and now ask him to cross his upper arm forcibly over his chest, so that the movements of his scapula and consequently of his humerus are restricted, we find that he can no longer rotate his forearm within the ring. He can still perform pronation and supination, but the ring must be allowed to move with his radius; in other words, his radius is alone moving, since the opposite movement of the ulna, which was necessary to diminish the curve travelled over by the radius, and so permit rotation within the ring, has been restrained simultaneously with the restraint of the humerus. Had the lateral movement of the ulna been chiefly in the elbow, or had it not come from the humerus, why should the restriction of the movements of the humerus have made so much difference in the mode of performing pronation and supination? As soon as the crossing of his arm is sufficiently relaxed to allow the scapula to move again, the rotation of the forearm can again easily be performed within the ring.

The careful observation of such a case as this seems to me to be of the greatest value in the investigation of the movements of the ulna in pronation and supination. Possibly there may be a slight lateral movement at the elbow-joint, but undoubtedly, as this and other similar cases show, the greatest part of the lateral movement takes place by rotation of the humerus on its own axis at the shoulder-joint.

Professor Chiene (who along with Mr Bell had been asked by the President to examine the case of anchylosed shoulder-joint) said he felt much interested in Mr Cathcart's investigations on this point. He thought the demonstration proved that this movement took place at the shoulder. If it were proved to take place at the elbow, then one of the most valuable diagnostic signs of disease of that joint was in danger of being lost. That fear, which had been present to his mind when Mr Cathcart first told him of his investigations, was now quite removed.

Mr Joseph Bell said he was quite convinced by Mr Cathcart's demonstration. The fact that the shoulder was not completely anchylosed in the case on which the movements were shown did not, he thought, militate against Mr Cathcart's argument, but was rather in its favour.

III. Clinical and Pathological Demonstrations.

1. *Dr P. A. Young* showed a CHART of abnormally LOW TEMPERATURE in a gentleman enjoying good health. He was a man of very studious habit, able to go through a great deal of work, and about fifty years of age. He suffered occasionally from indigestion, and

if he dined about six he had pains about two in the morning. He asked Dr Young to see him after an attack of influenza, which had left him rather weak. His heart was slightly weak, but there was no organic lesion. His temperature was between 95° and 96°. A tonic was ordered and rest. At the end of a fortnight the temperature was the same. The suspicion was then entertained that this might be the normal condition in this case. A series of observations was then made with the view of determining this. The temperature was taken in the morning, before dinner, and between 11 and 12 P.M. The morning temperature ranged from 94°·8 to 96°·8; before dinner from 95° to 96°·8; and at night from 97° to 97°·8, but never rose above that. The daily temperature curve differed from that given by Liebermeister, who stated that the temperature rose after dinner, but that after the last meal of the day there was no appreciable rise. He thought that the reason why his patient's temperature rose in the evening was that his digestion was slow, for, as they knew, the temperature continued to rise during the act of digestion. There was no sensation of cold on the surface of the body.

The President asked if the gentleman had much brain work.

Dr Young said he had, being a professor in the University, and his studies were chiefly philosophical.

Dr Argyll Robertson asked how the temperature was ascertained.

Dr Young—In the axilla. No mucous surface had been tried.

In answer to Dr Affleck, Dr Young said the pulse-rate averaged 74, and in reply to Professor Grainger Stewart, said the amount of urea excreted was not inquired into.

2. *Dr M'Bride* showed A NUMBER OF ADENOID GROWTHS removed from the naso-pharynx of a boy, æt. 9, who suffered from deafness and impeded speech, with snoring at night, and other symptoms of obstructed nasal respiration. The occurrence of these growths had been observed previously, but Meyer of Copenhagen deserved the credit of having first given a full and careful account of the signs and symptoms. The affection seemed to be much more common in some places than in Edinburgh, *e.g.*, Copenhagen, London, Norway, etc. This condition should be carefully distinguished from post-nasal catarrh, of which, however, it might be a result. The affection was one commonly seen in children and young persons, and depended in most cases on hypertrophy of the glandular tissue which was situated in the vault of the pharynx. Probably it tended to disappear at or after puberty, although the occurrence of adenoid growths has exceptionally been observed in elderly people. The diagnosis of this condition might be made with probability without physical examination, but to make certain, palpation and, if possible, inspection were necessary. The symptoms were:—(1.) Nasal obstruction; (2.) Deafness, usually

but not always present; (3.) Nasal speech, *e.g.*, common was pronounced with a tendency to substitute b and d for m and n. *Physical Examination.*—(1.) The absence of hypertrophied mucous membrane in the anterior nares and of enlarged tonsils in a child or young person complaining of the above symptoms, although, of course, two or all of these affections might be associated; (2.) An appearance of abnormal distance between the posterior wall of the pharynx and palate, as if the latter were kept down; (3.) Mucus flowing into pharynx from the posterior nares; (4.) Digital exploration. In most marked cases the finger when passed behind the palate came upon a soft, smoothly granular mass, which gave the sensation as if the finger could with but little force be pushed right through it. Sometimes pieces might come away, and the finger was almost always coloured with blood, however gentle the manipulation had been. Sometimes a number of the growths sprang from a much thickened, tumour-like base. (5.) Rhinoscopic examination was not so satisfactory in children, but in young adults it yielded definite results, the observer being thus able to appreciate the form and position of the growth; (6.) Water injected into one nostril did not flow out of the other. The treatment was too large a subject to discuss at length. In cases in which digital examination showed only the presence of velvety thickening of the posterior nares, without the presence of a sufficient amount of adventitious tissue to obstruct the nostrils, otherwise than by the associated catarrh, Dr M'Bride considered the application of alkaline washes and astringents sufficient. Indeed, he did not consider cases of this nature as more than post-nasal catarrh. Yet he could not but suspect that in some instances of this kind, when associated with Eustachian obstruction, operations were performed, and he believed he could explain the success which often attended such interference. Portions of mucous membrane being removed from the vault of the pharynx, the resulting cicatricial contraction not only braced and gave tone to the surrounding parts, but might actually tend to pull the lips of the Eustachian tubes apart. In advanced cases he used either cutting forceps or crushing (which he showed), both being less painful than operating with the finger nail. One point he wished to insist upon was that no violence should be used. Whatever did not come away easily should be crushed, and its vitality thus destroyed. The time he had already spent forbade further reference to treatment and after treatment.

Mr Joseph Bell said he did not think the condition to which Dr M'Bride referred was so rare as he supposed. He had for some time past been in the habit of describing it at his out-patient clinique. He found that many cases were benefited by the application of an astringent, as sulphate of copper, 30 grains to the ounce.

3. *Mr Joseph Bell* showed (*a.*) HALF OF A TONGUE removed for a

PAPILLATOMATOUS TUMOUR, from a man aged over 50, who had noticed it growing about four years before. The operation was done by Whitehead's method, and the edges of the wound in the mouth were united with sutures of catgut, which greatly facilitated the healing. The patient was out of bed on the third day, and left hospital on the eighth after the operation. (*b.*) A MYELOID SARCOMA OF THE LOWER JAW. Part of the tongue had to be removed along with the tumour. The patient had made a good recovery. (*c.*) AN ADENOID TUMOUR OF THE MALE MAMMA. In the male this was a somewhat rare tumour, more so than cancer. It was removed very carefully by a crucial incision, as he had recommended in a paper read to the Society a good many years previously. (*d.*) A GIANT FINGER, removed from a small child aged four months, of which Mr Cathcart had made a very beautiful cast.

4. *Mr Cathcart* showed the CAST of the GIANT FINGER which Mr Bell had just exhibited. It was taken in gelatine and glycerine by a method which he hoped to describe to the Society at a future date.

5. *Mr A. G. Miller* showed (*a.*) A MICROSCOPIC SECTION OF NERVE (prepared for him by Dr Bruce) from a case of tetanus. The patient had fallen a considerable height and sustained a compound fracture of the elbow. Two days after his admission, a portion of bone was removed to relieve tension and evacuate discharge. On the fourth day symptoms of tetanus began to show themselves, and notwithstanding amputation of the arm, the man died of tetanus and septicæmia. Examination of the arm after amputation showed the lower end of the humerus bare, with evidence of osteo-myelitis. There was a fracture through the internal condyle of the humerus; the ulnar nerve was much enlarged, about three times its ordinary size, round the condyle. It was very vascular, and on section showed a distinct extravasation in its substance. The musculo-spiral was also enlarged and vascular. All the nerves examined were found enlarged, with the bloodvessels distinctly marked upon them. Dr Bruce said the section he had made showed a great increase in the number of leucocytes in the perineurium, and a considerable increase between the nerve fibres themselves, a perineuritis running through the nerve. He did not make any examination for bacilli, because he was not aware of the condition of the patient when the nerve was handed to him, and it had not been preserved in an antiseptic fluid. The section was made by means of the microtome, which he intended to show at a later period of the evening. (*b.*) TEMPERATURE CHARTS OF TWO STRICTURE CASES, showing high temperature after instrumentation. The first was that of a patient who had a stricture, and was operated on by perineal section for rupture of the urethra, and had consequently a wound in the perineum when admitted into hospital. It was almost impossible to pass an instrument into the bladder.

Not wishing to do a second cutting operation, Mr Miller determined to dilate the urethra. On the first occasion of passing an instrument into the bladder (for which he was put under chloroform) his temperature rose to 102°·8 F., and the same evening was subnormal. Three days after an instrument was again passed. His temperature in the evening was 106°·2 F. It fell during the night, and at visit next day was almost normal, and that night was subnormal. Two or three days after, considering that the rise of temperature was due to a nervous cause, Mr Miller put the patient under chloroform during instrumentation, and the rise was simply half a degree. Quinine and morphia were given on after occasions, and the temperature rose but slightly. The other chart was from the case of an old soldier who had had fever in India. As a rise of temperature was expected in his case, he was given ten grains of quinine and a hypodermic of morphia, and put under chloroform. After a rigor the temperature rose to 103° F. At subsequent passings the temperature rose not at all, or very slightly. In spite of this high temperature, neither of these patients had any other symptoms indicating fever.

6. *Dr Byrom Bramwell* showed (*a.*) SECTIONS and DRAWINGS of the brain, the membranes of the brain, and the choroid in ulcerative endocarditis showing micrococci. Though meningitis was known to be a common symptom of this affection, these organisms had not been found in these structures before. (*b.*) A SECTION OF THE CHOROID in amyloid degeneration which he had incidentally come across in making observations on the optic nerve. This condition was considered rare. Gowers stated that Knapp had described it, but beyond that he did not know of any other references. It was quite probable that it was more common than was generally supposed, these parts not being usually subjected to a careful examination. (*c.*) AN ANEURISM OF THE THORACIC AORTA. This was one of the largest intra-thoracic aneurisms he had seen, which had not produced erosion of the walls. It involved on its anterior surface the vagus nerve. The patient was a man, aged 58, and 5½ feet in height. He was admitted to the eye-wards for an injury—dislocation of his lens. The aneurism was discovered incidentally, he having made no complaint of it. Next night when he was lying quietly in bed it ruptured and filled the greater part of the left thorax. The man had been employed for two years at the Forth Bridge Works. He did not appear to have had very laborious work, but the interest was that with such an enormous tumour he was able to do any work at all. (*d.*) PHOTOGRAPHS of macroscopic sections of the human brain. (*e.*) PHOTOGRAPHS of microscopic sections of the medulla oblongata, made by Dr Theodore Dekke, pathologist at the Utica State Asylum, and presented to him by Dr Palmer of that Asylum.

7. *Dr Alex. Bruce* showed a MICROTOME for cutting sections of

large organs. Working for over a year at the anatomy of the brain, and following the method of Prof. Hamilton, he had met with several difficulties. *First*, the process of freezing took three hours and required constant attention. After spending that time very frequently the whole process had to be gone over; then, again, the knife was very unsteady, and the screw which was underneath the table was so irregular that one cut sometimes a thick section, sometimes a thin section, and sometimes a section with a hole in it. Dr Bruce applied to Mr Frazer in Lothian Street to construct for him a large instrument somewhat on the plan of the Swift microtome, in which the ice and salt should be put in a large box and the heat abstracted downwards by means of pillars. With this instrument he could freeze a piece of tissue half-an-inch thick sufficiently to enable him to cut very perfect sections. Further, the tissue remained frozen a much longer time than it did with the other microtome. It was necessary to devise a knife which might be brought down to the section. A system of rails cast in gun-metal had been devised, and working on these rails this somewhat heavy looking instrument, the half of an ordinary guillotine knife such as bookbinders used to cut paper. It was thought of because of its stoutness and the fine temper of its steel. The action of the instrument had far exceeded his expectation. It worked so well that he had been able to cut small sections with more accuracy than with Swift's or the freezing microtome.

8. *Dr Milne Murray* showed (*a.*) An improved form of the "ARTERIOGRAPH," an instrument which he had described to the Society on a previous occasion. (*b.*) A simple and cheap form of RECORDING DRUM for use with the arteriograph, and he pointed out that such a drum might be found convenient for clinical observation with the sphygmograph or cardiograph. (*c.*) A number of TRACINGS obtained from the carotid of a rabbit by means of the arteriograph. These tracings showed clearly the effect on the arterial system produced by stimulation of the following nerves:—1. The trunk of the vagus; 2. Central end of the divided vagus; 3. Peripheral end of the divided vagus; 4. Trunk of the depressor; 5. Trunk of the sympathetic. Dr Murray drew attention to the remarkable similarity between the tracing obtained during stimulation of the vagus peripheral end and that obtained in cases of aortic incompetency by Marey's sphygmograph. These tracings, however, showed one important difference which he thought worthy of attention. In the sphygmogram from aortic regurgitation the dicrotic wave appeared low down in the descending line, and was a mere undulation with a blunt contour. In those obtained by means of the arteriograph during stimulation of the vagus the dicrotic notch occurred almost immediately after the "tidal" wave, high up in the descending line, and was represented by a sharply defined wavelet, with a distinct upstroke and pointed crest. This difference would seem to confirm the views of those observers who held

that the dicrotic notch was mainly due to a recoil wave from the aortic valves. In cases of aortic incompetence this recoil would be much reduced in intensity and delayed in transmission. Quite probably it might in those cases result from a succussion against the ventricular walls. Hence its appearance towards the end of pulse-wave and its imperfect development. In vagus stimulation they had a certain portion of the phenomena of aortic regurgitation reproduced, such as great emptying and sudden filling of the arterial system due to the cardiac inhibition; but, on the other hand, the aortic valves being intact, the recoil wave would be produced early in the tracing, and might be expected to appear as it did, in these tracings near the top of the crest of the sphygmogram.

Meeting XII.—July 1, 1885.

Dr H. D. Littlejohn, *President, in the Chair.*

I. Election of New Members.

The following gentlemen were elected Ordinary Members of the Society:—George Kerr, M.A. Oxon., M.B. Edin.; Tom Bairstow, L.R.C.P. & S. Ed.

II. Exhibition of Patients.

1. *Prof. Grainger Stewart* showed (*a.*) a MAN afflicted with DEAFNESS, which he believed to be due to a lesion of the auditory centres. He was in perfect health till the day of the Volunteers' Review in 1881. On leaving the ground he fell on the back of his head. At night, having recovered from the effects of his fall, he went out and fell again. He was brought in unconscious, and with symptoms of fracture of the base of the skull. He remained completely unconscious for a time, after which he was deaf. This was not due to any defect in the ears themselves. It might be due either to an extraordinary injury of the auditory nerve endings, the nerves themselves, or the nerve centres. He thought that in all probability the centres themselves were affected. He admitted that fracture of the base might have injured the end apparatus or the nerves, but as no other cranial nerve was injured this seemed unlikely. He showed, in some measure, the peculiarity in speech which deaf-mutes who had been trained to speak usually showed. He had no perception of sounds except by sometimes feeling the vibrations. (*b.*) A FATHER AND TWO SONS affected with disease of the spinal cord. For having these cases in his wards he was indebted to the courtesy of Dr Macphail of Whifflet, who had carefully studied them for several years, and two or three years ago had brought them before one of the Glasgow societies. The father's case presented certain peculiarities. It appeared that for about eight years he had been the subject of

slight paralysis, particularly in the left leg. Afterwards, changes began to show themselves in the right leg also. There was that over-irritability of the muscles, with the tendency to increased deep reflexes, ankle clonus, etc., which marked lateral sclerosis. This went on and culminated in contraction of the flexors at the knee-joint, which was so great as to prevent the knee being stretched. Along with this condition there was abundant evidence of slight affection of the left side of the body, and marked affection of other parts of the right side. There were no sensory phenomena of any importance. He was now unable to work. It was evidently a condition of lateral sclerosis they had to deal with, whether primary or descending, following on some injury he might have received at his work as a miner, he (Prof. Stewart) could not say. It was certain that he had received an accident which had precipitated the symptoms. Whether it had caused them he did not know. The patient had not in his own family, so far as could be made out, any particular tendency to paralysis. His father had died of apoplexy, but there was no history of any other nervous disease among his relatives. The two boys, who were also shown to the Society, exhibited peculiar symptoms, in part corresponding to pseudo-hypertrophic paralysis, in part suggesting a spastic condition. The elder of the two, who is about 13 years of age, is, as you see, quite helpless. Some of his muscles are abnormally large, those of the calf for example, certain muscles about the jaw, the floor of the mouth, and the tongue. Others are much atrophied, such as the biceps of the arms, the pectorals, and the erectors of the spine. In consequence of this weakness the body is much deformed, the spine is curved backwards, so that the chin is nearly in contact with the abdomen, the legs are bent up, the feet in the position of talipes equinus. The patient has also developed certain special manœuvres, in order to effect movements to which his muscles are no longer equal. For example, when wishing to raise his right arm, he catches his fingers with his teeth, and pulls the hand up. The organic and skin reflexes are natural, the deep reflexes are lost. The electric irritability of the muscles, as a rule, is diminished both to faradism and to galvanism, but one or two of the muscles showed excessive excitability. The sensory functions are normal. The vasomotor shows no abnormality except in the muscles. The intelligence is acute. The younger boy, who is about 11, is able to walk about. He stands pretty steadily, has not the characteristic straddling gait, and shows at present no abnormality, except in the legs. He has during the past four years gradually developed a distinct talipes equinus, and it is now more marked than one usually sees it in advanced cases of pseudo-hypertrophic paralysis. The muscles of his calves are enlarged and firm. There is no evidence of paralysis or wasting of the muscles in the front of the leg. His organic and skin reflexes are natural, but the tendon

reflexes are markedly increased at the knee-joint, and some ankle clonus can be occasionally made out. The case thus shows certain affinities to spastic paralysis, and may turn out to be, like the father's, an example of that condition, rather than, like the brother's, of pseudo-hypertrophic paralysis. His sensibility and intelligence are natural, but there is a tendency to mottling of the skin of the feet and legs, and, as Dr Macphail has pointed out to me, this boy and the father show a tendency to alopecia areata, which does not exist in the other boy. Another point of interest is the family history. The father's history shows no special tendency to nervous disease; the mother is a healthy and vigorous woman. She, however, had an uncle and a cousin who suffered from a chronic form of paralysis, and of the nine children whom she has had, none of her three girls has shown any tendency to disease, but at least four of the boys have died of, or are suffering from, nervous symptoms. The father states that of the two who died one was more like the pseudo-hypertrophic, and the other more like the spastic condition exhibited by the patients now shown. The cases appeared to Professor Stewart of such interest that he had asked Dr Philip to investigate them more fully with a view to their complete description.

2. *Dr Allan Jamieson* showed a PATIENT whom he believed to be suffering from MORPHŒA. He was a man, aged 56, who had consulted him a few days before at the Royal Infirmary, having been sent by Dr Alexander of Wick. The condition seen on the patient's head commenced eight years before by the eruption of one of the hard nodules, a number of which were seen on the scalp. This disappeared, and left no trace beyond a slight violaceous colouring of the skin. Other nodules appeared, which also disappeared in part. There was a number of them in process of involution. They were of a firm consistence, but not hard. Some were fixed to the skull, others were quite movable. At one point a deepish ulcer had formed with edges which were sharply defined. It had excavated almost down to the bone. Round the nodules were violaceous patches, and these also replaced former elevations. There were four conditions which might produce this—(1.) Syphilis, of which in this case he had not been able to find any evidence, nor had Dr Alexander. He had married only two years ago, and had no family. (2.) Epithelioma. The ulcer was very like an epitheliomatous ulcer, though it had not the hard margins. Against the theory of epithelioma there was the fact of the absorption of the nodules, leaving no cicatrix, but only a purplish discoloration. (3.) Leprosy. He had, however, never been away from Wick, had not been to Norway, and none of his family had had any such affection. There was no anæsthesia nor abnormal sensations in the nodules. The face was perfectly healthy. Again, Dr Bruce had examined a piece cut out from a nodule, and looked

for the bacillus lepræ and had not found it. It must, therefore, Dr Jamieson thought, be excluded. (4.) Morphœa. The characters of the disease approached that affection very closely. They had the peculiar waxy infiltration of the skin, and though the nodules rose upwards, this might be accounted for by the fact of its occurring on the scalp, so that the infiltration was forced to extend outwards. Then they knew from Hutchinson's observations that morphœa disappeared, underwent spontaneous cure, and left a purplish discoloration. The microscopic examination was also favourable to the view of morphœa. There was entire obliteration of the papillæ; the under surface of the rete mucosæ was quite smooth, and in the papillary layer there was an infiltration of small cells. These appearances corresponded with those described by Dr Radcliffe Crocker. The only thing against the view of morphœa was the ulcer, which so far as he knew had not been described as occurring in morphœa though it did exist in scleroderma. It was, however, possible that degenerative changes might take place, and there might be an epitheliomatous change going on at the ulcer, which he hoped to determine by a microscopic examination of a portion of the edge. He was bald when the affection began. It was known that leprosy lingered longer in the North of Scotland than elsewhere in the country, and Sir Erasmus Wilson was of opinion that morphœa was the representative in the present day of that disease, so that this affection might be a sort of cousin of leprosy.

III. Exhibition of New Remedy.

Dr Allan Jamieson showed a new oil which promised to be very useful in some cases of skin disease. It was prepared by the Dee Oil Company, and was a very heavy hydrocarbon oil. It did not seem to irritate, and might in that respect be considered a liquid vaseline, but purer. It was especially useful after alkaline baths in psoriasis. In the *Practitioner* for June there appeared an account by Dr Roberts of Chester of forty-two cases in which he had employed this oil, to which he gave the name Oleum Deelinæ. It appeared to be palliative and soothing. It was cheap, quite odourless, and apparently very easily absorbed, inasmuch as the oiliness disappeared in a very short space of time after its application.

IV. Original Communication.

THE PLACE OF SPECIALISM IN GENERAL PRACTICE, WITH REFERENCE TO DISEASES OF THE EYE, EAR, AND THROAT.

By George Hunter, M.D., F.R.C.S. Ed., Linlithgow.

Twenty years ago the diseases which form the subject of this paper received but a small share of the attention which is now

devoted to them in this Medical School. It is true that a clinical course on the diseases of the eye was held in the wards of the Royal Infirmary, but it is doubtful if many utilized the opportunity of there becoming practically instructed in the ordinary ocular affections; and it is certain that the great majority of the students at the time referred to, possessed but little if any acquaintance with the estimation of the errors of refraction, or were able to make use of the ophthalmoscope as a means of diagnosis.

So far as I am aware, there was no course of instruction, either in the University or at the Extra-mural Schools, on the diseases of the ear and throat, it being taken for granted that in the systematic lectures on medicine and surgery the maladies to which these organs are liable would find a place—which, I need scarcely add, they rarely did; or if so, a very unimportant one.

Need it cause surprise, then, that men went into practice having a very imperfect knowledge of the special diseases referred to, and that, in order to supply their deficiency, it became necessary to proceed to the medical schools of Vienna, Berlin, and elsewhere abroad. In the cities no difficulty resulted, for the services of a specialist were always within reach in any particular case; but in the smaller country towns, and especially in country practice, these cases were practically ignored; or if recognised, passed on, it may be at much inconvenience, to those known to have made them the subjects of special study.

Such was the position of matters when, without having had the advantage of any special course of instruction, I commenced practice. Very soon afterwards I was consulted by Mrs S., a country patient, who complained of defective sight of one eye. External examination afforded no explanation; and in my difficulty I sought the advice and assistance of my predecessor, who made a careful inspection, and came to the conclusion that she was affected with cataract. Dissatisfied with our combined skill, she visited a well-known ophthalmic surgeon, who, as she subsequently informed me, examined her in the dark room, and promptly informed her that the nerve had got separated from the back of the eye, and that there was little prospect of her sight improving.

About the same time Mr A. B. sought my advice regarding an intermittent discharge from one ear which at times became offensive, and which was accompanied by considerable impairment of hearing. To cure what was regarded as a "simple suppurating ear," astringent injections were prescribed, but progress being tedious and unsatisfactory, I accompanied him to a surgeon then noted as an aurist—now one of our most distinguished ovariotomists—who examined with mirror and speculum, and demonstrated with precision a perforation of the *membrana tympani*, the existence of which I was previously unaware of.

Some time afterwards Mr J. M. complained to me of hoarseness and dysphonia, which he said had lasted for a considerable time,

and which caused oppressive dyspnœa on exertion and in damp foggy weather. Counter irritation was employed externally, and the spray of *sulphurous acid*, then in the zenith of its popularity for all throat affections, diligently made use of. He did not improve, however, and having occasion to be in London, he consulted a throat doctor, who used the laryngoscope, and after asking one or two leading questions, appeared speedily to have gained all the information he desired, and remarked to an assistant who was present—"A case of T. S.," *i.e.*, tertiary syphilis.

These three may be taken as fair examples of the cases that may appear in the consulting-room of most general practitioners, and I venture to question if the great majority of those in ordinary practice would recognise their exact nature. On the other hand, the rapidity and ease with which the specialist makes his diagnosis is calculated to produce in the mind of the patient an unfair, because exaggerated, idea of his ability, and of the lack of it in the case of the ordinary attendant, not merely in the instances referred to, but in medical matters generally.

The necessity of knowing something more of these ailments became firmly impressed on my mind if the confidence of my patients was to be retained, and in bringing this communication before the Medico-Chirurgical Society, I wish to disclaim any aspirations after specialism, and to indicate, by narrating short notes of cases, what affections of the eye, ear, and throat those of of us who are in general practice may reasonably be expected to recognise and treat. In so doing I must crave the indulgence of those members who are already well known in their special departments, and to whom what follows may appear, if not tedious, elementary and disconnected. And first, with reference to

Affections of the Eye.

Eye surgery, with but few and unimportant exceptions, cannot be entrusted to the general practitioner. The late Mr Liston used to remark that before the surgeon succeeds in successfully operating for cataract he must destroy a hatful of eyes; but, without subscribing to this pithy opinion (which would require much modification since his day), my belief is, that the fewer the attempts on the eyeball itself the better for the patient and his own credit. The operation for convergent strabismus is not unfrequently so performed; but I doubt much if, in most instances, the morbid conditions which underlie it are understood or recognised, and if the operator, after dividing the tendon of the internal rectus, takes into account the amount of hypermetropia present, how far back the new insertion should be placed, and the necessity of also shortening the tendon of its fellow of the other eye.

The same difficulties do not present themselves in the operation for obstruction of the nasal duct, and in ordinary cases do not

require special skill, as the following case will show:—Mrs L. consulted me last autumn for inflammation of the lachrymal sac with *epiphora*, the consequence of stenosis of the nasal duct. Having introduced the director into the canaliculus, I slit it up to its full extent with a small tenotomy knife, and passed a Bowman's probe through the stricture. Finding those I used too small, I introduced the olivary pointed ones recommended by Mr Couper, and since using them I have been able to keep the duct patulous, with the result that the lachrymation, etc., have almost entirely disappeared. In the obstruction of both ducts, necessitating the dextrous use of both hands, the patient had better be placed under the care of the ophthalmic surgeon.

The *corneal affections* not only prove the extreme risks to vision which they are apt to give rise to, but also from the difficulty, anxiety, and tedious course of treatment which they occasion, must always be of great interest to the practitioner.

There are few of us who have been long in practice who cannot recall the disastrous results—in the shape of dense leucoma, anterior staphyloma, and perforation, with subsequent conical cornea and irregular astigmatism—for which they are responsible. It appears fitting, therefore, that they should be here briefly alluded to; for I believe there is no class of eye diseases which causes more heartburning between the patient and his friends, or in which the skilled opinion and treatment of the specialist are more urgently solicited than in ulcers—phlyctenular and otherwise—of the cornea. So generally is this opinion entertained, that a committee of the London Ophthalmological Society, at the instance of Mr Jonathan Hutchison, recently formulated a line of treatment for the guidance of surgeons in general practice. The following case will serve as an example of the kind of treatment I have found most successful in phlyctenular ulceration of the cornea:—R. W. came to me in March last suffering from great photophobia and lachrymation, and no persuasion would induce him to allow the lids to be separated and a view of the eyes obtained. A drop or two of a 4 per cent. solution of cocaine was instilled, and in a few minutes afterwards I was able to make examination of the eyes. I found strumous ulceration of the cornea and conjunctivitis, with slight vascularity leading up to the ulcerated surfaces. A lotion of boracic acid in combination with atropine, and a morsel of the yellow oxide of mercury ointment to be placed within the lower lid at bedtime, were prescribed (the latter having been employed by most of us long before Mr Hutchison recommended its use). The internal treatment consisted of minute doses of arsenic in combination with syrup of the iodide of iron, and general anti-strumous treatment, the result being rapid improvement and recovery, with very slight remaining opacity. I cannot too strongly recommend the use of small doses of arsenic in such cases.

Before entering upon the question of the use of the ophthalmo-

scope, the importance of focal illumination must be briefly alluded to. Foreign bodies and opacities of the cornea, and changes in the iris and lens, are thus easily recognised. Last autumn a patient came to me complaining of something having got into his eye, and on using the large condensing lens exhibited, I easily detected, embedded in the cornea, a small fragment of straw, the result of a sheaf of wheat having been thrown in his face. I steadied the ball with fixation forceps, and readily extracted it. In this patient no cocaine was used; but the following case well illustrates its beneficial anæsthetic effects:—J. D. came to me a week ago having his right eye severely burned with boiling solder. He was suffering much pain, and could not allow the eye to be touched or the lids separated. Two or three drops of the 4 per cent. solution were instilled at short intervals, and thereafter pieces of solder, forming partial casts of the upper and lower conjunctival reflexions, were removed without the slightest pain being complained of.

In the diagnosis of cataract focal illumination is indispensable; and had it been employed along with the ophthalmoscope in Mrs S.'s case, retinal detachment would not have been mistaken for lenticular change, whether nuclear or peripheral.

Of the nature of the *ophthalmoscope* in the hands of the ophthalmic surgeon every one is now fully cognizant; but judging from my own experience, few practitioners in general practice avail themselves of the important aid its use is capable of affording, not only in those affections which are purely ocular, but in such diseased conditions as syphilis, tubercle, anæmia, renal cirrhosis, and diseases of the spinal cord. In diseases of the nervous system it finds its most important application, and here the writings of Clifford Allbutt, Hughlings Jackson, Buzzard, and Gowers, in this country, and of Charcot in France, have done much to incite the interest of the profession towards its more general employment. Dr Drummond[1] writes, "The optic apparatus should be most carefully investigated in all cases of suspected brain disease. No one can hope to make progress in the diagnosis of cerebral disease without some knowledge of the ophthalmoscope; indeed, it is a *sine qua non* in nervous diseases, and may be said to be of as much importance in brain diseases as the stethoscope is in thoracic disorders." Dr Buzzard,[2] in alluding to a case of paraplegia which had been regarded as very obscure by the former medical attendant, who had treated the patient ineffectually for three months, states, "On using the ophthalmoscope (which we employ, thanks to the wise teaching of Dr Hughlings Jackson, as a matter of routine), we found in the right eye very evident remains of disseminated choroiditis in the form of numerous atrophic and pigmentary spots, whilst in the left were old adhesions of the iris, and opacities of the vitreous body. There was no history of bad sore

[1] *Diseases of the Brain and Spinal Cord*, p. 62.
[2] *Diseases of the Nervous System*, p. 406.

throat or eruption of the skin. From the beginning to the end of this case no direct question was asked of the patient or her husband in reference to infection. The ophthalmoscopic appearances pointed to constitutional syphilis with such force that the corroborative evidence afforded by the immediate influence of iodide of potassium was scarcely needed to establish the specific nature of the lesion from which this woman suffered.

Charcot,[1] writing of the importance of the early diagnosis of optic atrophy and its significance, states, "According to some ophthalmologists, the optic lesion proper to ataxia may, in a certain number of cases, precede all the other symptoms, and of itself constitute the whole disease, often during many long years. Now nothing is better established in my opinion, as I have already indicated, than the accuracy of this proposition. . . . Hence it is important to be able to recognise the identity from the beginning of this affection of the optic nerve, which, ten or fifteen years after its invasion, shall be followed by ataxia."

Dr Gowers,[2] in remarking on the value of the ophthalmoscope in diagnosing tubercular meningitis, writes, "In some cases the cerebral symptoms are latent or dubious, and in these the examination of the eyes may afford very valuable help, and it is probable it would do so in at least one-third of the cases. Of twenty-six cases watched by Dr Garlick, the ophthalmoscope was of real diagnostic assistance in six, and probably would have been so in a larger number had earlier examination been practicable. In one case which had lasted twenty-six days, the symptoms were indefinite until the nineteenth day, whereas on the fourteenth the ophthalmoscopic changes were so unmistakable that the diagnosis of meningitis was confidently made."

In the diagnosis of renal disease its use may be of equal importance.

Mrs J. came to my consulting-room last November complaining of failure of vision of both eyes. Ophthalmoscopic examination disclosed double optic neuritis, with large patches of white exudation around the *macula*. Here, in the absence of any specimen of urine being at the time obtainable, a confident diagnosis of chronic Bright's disease was at once made. Moreover, those cases of renal cirrhosis where no reaction of albumen can be obtained by its precipitants for days together, the discovery of albuminuric retinitis furnishes information of the utmost value to the physician as to diagnosis and prognosis, and that in a manner as delicate as it is trustworthy.

From what has been stated, I trust it may be conceded that an acquaintance with the use of the ophthalmoscope may be of the greatest advantage to the medical practitioner in his daily round of practice; and I am sure the confidence of his patients will not be lessened by its systematic and regular employment.

[1] *Diseases of the Nervous System*, pp. 36, 37.
[2] *Manual and Atlas of Ophthalmoscopy*, p. 142.

Regarding the method of examination, the *indirect* is the more generally employed, especially by the ophthalmic surgeon; and it possesses manifest advantages, for a much more general and extensive view of the *fundus oculi* can be obtained, and no special lenses are required in cases of abnormal refraction. In medical ophthalmoscopy, however, it is essential that the direct method be also had recourse to, and for the following reasons:—(1.) It can be used without having the pupil previously dilated with atropine (and few of us are aware, unless it has been experienced, how much discomfort results from having the accommodation paralyzed for several days together). (2.) Minute changes of structure and morbid appearances are readily recognized which would escape observation by the indirect examination.

For the former, the simpler the instrument the better. I have used a Nachet ever since the early days when a view of the optic disc was the goal of my ambition, and the fundus oculi a *terra incognita* to be diligently explored.

For direct examination I have used those of Landholt and Couper, and I unhesitatingly prefer the latter, principally because it furnishes such an excellent, accurate, and easy means of estimating the refraction (which is in so many cases anomalous), but also because it enables the observed and the observer to be placed less directly face to face, and feature to feature, to the obvious comfort of both.

The first part of the examination will reveal the presence or absence of opacities in the media, whether the red glare of the fundus comes into view, and if a blurred image of the vessels can be seen at from 18 to 20 inches or more from the cornea; and this leads up to the important question of the estimation of *refraction* by the general practitioner. One only requires to take up a provincial newspaper to find in its columns advertisements of lenses and preserves guaranteed to remedy all visual defects, and to notice posted on the walls of our railway stations and elsewhere female faces spectacled with "aqua-crystal" and other glasses, to realize that to a great extent this matter has been taken up by advertising quacks and ignorant and irresponsible persons. To quote Dr Brailey, "These people insist that two or more pairs of spectacles are necessary, and extract several pounds for what they are incompetent to judge correctly. It is high time, therefore, that general practitioners should acquire that amount of knowledge which would enable them to prescribe for themselves." With this opinion I desire to express my complete concurrence, and to state that, not only should simple and uncomplicated cases of myopia and hypermetropia be recognised by the ordinary medical attendant, but he should also be competent to estimate the amount, and prescribe the correction necessary. Especially would this be desirable at a distance from the cities, and in the case of the poor.

With Snellen's test types for distance and those for reading in the hand, a short series of convex and concave lenses (such as is found in Mr Nettleship's little pocket case), and a good refraction ophthalmoscope, this may be pretty generally and fairly accurately accomplished.

It would be out of place here to go into the detailed examination necessary for estimating the amount of ametropia in any given case. Such excellent instructions are given in the works of Hartridge, Morton, Juler, and Swanzy, that the opportunity of becoming acquainted with these methods is within every practitioner's reach. Nor shall I detain the Society relating instances of myopia and hypermetropia, in the greater number of which I have been able to prescribe successfully the required correction, but I shall now refer to those cases of anomalous refraction which are practically unrecognised outside the field of specialism, which the spectacle dealer can suit neither with his convex nor his concave lenses, and which give rise to more troublesome subjective symptoms than the profession generally is aware of. I mean the astigmatic.

Four years ago I was consulted by Mrs H. for headache, which became exaggerated on attempting to read or sew, and which ultimately became so intolerable, especially in the evenings, that she was obliged to give up reading altogether. Her head became confused and giddy, and she began to fear lest her brain should become upset. Aperients were at first prescribed, then alteratives, and, finally, large doses of bromides with general tonics, but without finding much improvement result. Fearing I might have a case of organic cerebral disease to encounter, I made an examination of the fundus oculi for optic neuritis or other changes; and, after having used the direct and indirect methods, and tested her with the horizontal and vertical lines in Mr Nettleship's case, I came to the conclusion that she was astigmatic. I then sent her to a well-known ophthalmic surgeon, who found her refraction to be hypermetropic astigmatism, and prescribed the necessary convex spherical combined with convex cylindrical lenses—

$$\left|\begin{array}{l} +2 \text{ D spherical.} \\ \hline +2 \text{ D cylindrical, axis horizontal.} \end{array}\right.$$

It would be over-estimating the benefit which resulted were I to state that the disagreeable feelings in the head at once disappeared; but she was able to read and sew without discomfort, the asthenopia being practically cured. Here the recognition of the ametropia saved the patient from a prolongation of the treatment by drugs, and directed where she could obtain the necessary and appropriate correcting glasses.

I was recently consulted by Miss J. R., 14 years of age, who complained of severe pain over her left eyebrow, coming on every morning at the same hour (which was soon after commencing

lessons at school), and which became so severe that the eye got suffused, and she was obliged to leave off reading and return home, where, after a time, the pain gradually ceased. For eight weeks she had been obliged to absent herself from school on this account. Regarding her case as one of neuralgia, I prescribed quinine in combination with iron, and the regimen suitable to such cases; but without observing the usual benefit to follow. By chance, during one of my visits, I discovered that with the left eye she was unable to tell the hour on the town-clock (which was quite at hand), and I then hoped I had found the explanation of the pseudo-neuralgia. On testing her acuteness of vision, I found that with the right eye she could read $\frac{6}{12}$, with the left $\frac{6}{60}$. A weak convex made matters worse, but -1 D. brought vision up to nearly $\frac{6}{6}$ for the right, and with $-4{\cdot}5$ D. she saw very much better with the left, $=\frac{6}{9}$. On placing Snellen's "sunrise" before her, it was found that the vertical rays were much darker and more sharply defined than the horizontal, and myopic astigmatism with unequal refraction of the eyes, *i.e.*, anisometropia, was diagnosed. This was confirmed by an ophthalmic surgeon, who was good enough to prescribe for her suitable spectacles, with the result that her neuralgic pain was completely cured.

I might easily multiply cases like these from my own recent experience, to show how important it is that astigmatism should be recognised in general practice; for, in the cases related, it furnished a rational explanation of an appropriate treatment, and prevented an erroneous diagnosis of cerebral disease in the one case and of neuralgia in the other, where, *prima facie*, it might have easily been made.

To expect the ordinary medical attendant to prescribe the correcting lens in cases of astigmatism would be asking too much of him, especially as the cost of a trial case of lenses is something very considerable; but what I contend for is, the desirability of his being able to *recognise* them as they come under his notice, and having done so, to send them to the specialist.

I have already referred to the importance to the practitioner of an acquaintance with the diseased conditions of the *fundus oculi* as revealed by the ophthalmoscope, and this paper would fall short of the original purpose I had in view in preparing it if I omitted to accentuate what has previously been stated as to its value, by briefly narrating one or two cases where its employment cleared up difficulties of diagnosis, made prognosis more certain, and furnished indications calculated to be of service in treatment.

J. C. consulted me five years ago, then 9 years of age, for increasing difficulty in reading and preparing his lessons, for which he suffered at the hands of his teacher. External examination of the eyes revealed nothing to explain his amblyopia, nor was there anything in his previous health to account for it. Three

years previously he had had an attack of measles, and this was followed by acute cystitis of some severity. Ophthalmoscopic examination showed well-marked double optic neuritis, with enlarged tortuous veins, slightly diminished arteries, the disc being of a bluish-red tint, with indistinct, woolly margins. So far as I can recollect, there was no other change observed either in the retina or choroid at the time. He was at once taken from school, ordered to have quiet exercise in the open air, and simple but good nourishing food, with, in addition, cod liver oil. A careful watch was kept for any symptoms of tubercular meningitis, but none appeared, nor did repeated examination of the urine discover any albumen. The diagnosis of neuritis was confirmed by those competent to judge, and an unfavourable prognosis, *quoad* vision, given.

Five days ago I had the opportunity of again examining him, and found the following appearances:—The optic disc was of a chalky whiteness, the margins abrupt and well defined, but ragged and unevenly cut, and the vessels, which were not much altered in calibre, pursuing a straight course from the *papilla* to the retina, and giving no impression of prominence or excavation. In the immediate vicinity of the *macula lutea*, and surrounding the disc, the fundus appeared to indirect examination deeply pigmented, which was all the more distinct in contrast with the pallor of the disc; while to the direct it was of a coarsely stipled or granular black and white appearance. Towards the periphery the choroidal epithelium was atrophied to a certain extent, with large patches of pigment here and there; but here the fundus was of a healthier and redder tint. He could only read Jaeger 23 close to his eyes, and central vision was almost completely abolished, peripheral being fairly good. He could see better in the dark than the other members of his family.

In this singular case, in which the etiology is obscure, the only available information was furnished by the ophthalmoscope, and the direction and extent of vision are sufficiently explained by the ophthalmoscopic appearances.

The case of Mrs J. has already been briefly alluded to. Here the disc was of a lilac gray colour, with blurred margins, both arteries and veins being diminished in calibre. The striking features were the large patch of exudation round the macula, and the abundance of retinal hæmorrhages which were in linear and irregular patches.

The urine contained a large proportion of albumen, as was subsequently ascertained; the cardiac action was palpitating; and she had attacks of exhausting epistaxis. A very unfavourable prognosis was given to her friends, and a fatal termination occurred in less than two months from the date of my examination.

The only other case I shall briefly refer to is that of M. W., who came to me desiring to have spectacles ordered for her very defec-

tive sight. She was very slightly myopic, and on examining the fundus I found the disc atrophied, the vessels much diminished, and the degenerative retinal changes indicative of retinitis pigmentosa, to which the atrophy was probably secondary. To have buoyed up this poor woman's hopes with the delusive idea that any form of spectacles could have restored her sight, would have been to expend her hardly earned money for what was almost valueless to her; and her confidence in the prescriber's skill in such complaints would have been materially and deservedly lessened.

If it were necessary to adduce further evidence (from my own experience) to give point to my argument, or to strengthen the position I have endeavoured to establish in the preceding observations, I might refer to the importance of the early recognition of glaucoma, in which unrelieved and progressive intra-ocular extension leads to irreparable blindness, but where a timely iridectomy at the hands of an experienced ophthalmic surgeon permits a very hopeful prognosis to be given.

In bringing this part of my communication to a close, I desire to express my sense of the indebtedness I owe to those gentlemen who have made eye diseases their special study, not only for the valuable assistance and advice they have so willingly given me in the treatment of difficult and anxious cases, but also for the unvarying courtesy I have experienced at their hands.

The President said the Society was much indebted to Dr Hunter for coming to read them such an able paper. There was one important matter to which it pointed, and that was the improvement of their medical education. Either the Scotch student must gather his little and go to the Continent for instruction in such specialties as Dr Hunter had referred to, or else they must give it him at home. They must remember that Scotland, though increasing in wealth, was essentially a poor country, and many of their students were the sons of hard-working parents, who stinted themselves to enable their sons to be educated, and it was of importance that they should be able to give them a complete education in this country. The effect of Dr Hunter's paper would be to help that movement, and that the teaching should be improved and extended.

Prof. Grainger Stewart said the President had so eloquently put the thoughts that had arisen in his mind during the reading of the paper, that he had left very little for him to say. It had been his privilege for a good many years to know a good deal of Dr Hunter's practice, and he had been struck throughout by the admirable thoroughness with which every case was studied, and he had observed also the time and care given to these special studies in the work of one of the busiest lives in the whole of Scotland. The members of the Society must have been struck with the modest way in which Dr Hunter had kept himself in the

background throughout his paper. He felt with their President and Dr Hunter in regard to the importance of students being taught the use of the instruments required for the diagnosis of the special diseases to which reference had been made. He started some years ago a course of instruction in connexion with his chair on the use of these instruments in so far as they were required by physicians. He felt it was his duty to give every member of his class the opportunity of learning to use, first on the model, then on the living subject, the ophthalmoscope and laryngoscope. This work was carried out by Dr Philip. He believed that Dr Argyll Robertson and others found that the foundation thus laid for the students was of great use when they came to study the special diseases, that their work in teaching was to a certain extent facilitated. He hoped the time would soon come when no student would be allowed to leave Edinburgh without having acquired an acquaintance with the ophthalmoscope, the laryngoscope, and the application of electricity in disease.

Dr Argyll Robertson had little to add to what had been said by the President and Prof. Stewart in corroboration of the views expressed by Dr Hunter in his paper, these being what he himself entertained as regards the necessity for further instruction in special departments being insisted on of every student. He was glad that Dr Hunter's description of what obtained twenty years ago did not characterize the present day. The opportunities of the present were greater, and students were much more willing to take advantage of them. Though the class he taught was voluntary, it had been very considerably taken advantage of, and during the last ten years he had lectured, the average of the numbers attending had been at least fifty, indicating that the students were themselves aware of the advantages to be obtained by attending such lectures. He trusted that ere long, either by lengthening the course, or by relegating some of the classes which had a less practical bearing on the non-professional work to the position of preliminary subjects, these might form subjects a certain amount of knowledge in which should be considered essential before the student obtained the right to practise in this country. Unless this was done, they would fall far behind their Continental brethren, because at the present day in most of the European states no qualification was given to a student who had not some acquaintanceship with ophthalmology. In Germany, France, Italy, Russia, Austria, Norway, and Sweden, this obtained. It was high time that it should be made a compulsory subject in this country also.

Prof. Stewart asked if he might be allowed to supplement what he had said in the direction of Dr Argyll Robertson's remarks. He had very strong opinions on the desirability of introducing a number of subjects into the curriculum and making them compulsory. It seemed to him that the true way to do

this would be to make the courses on these subjects partly systematic, but chiefly clinical. Take, for example, diseases of the mind. He thought every student should be compelled to attend a course of twelve or fifteen lectures on this subject at a fee of say one guinea. This course would be largely illustrated by cases, would be chiefly clinical. Diseases of the eye were too important to be treated in such a manner, but he should like to see something analogous to it in the case of other subjects not included in the curriculum. He hoped the University clinical lectureships in the Sick Children's Hospital, and one which he hoped might be instituted in the new Fever Hospital, would be made before long compulsory, and he trusted the President would aid the University in the establishment and management of the latter proposal.

Dr M'Bride said he wished to refer to the question of how these special subjects should be taught. He had thought the matter over, and had come to the conclusion that a method somewhat analogous to that proposed by Professor Stewart should be adopted. The student should be instructed how to manipulate, and taught what was known of the disease at the same time. The typical course would be one of twelve or fifteen lectures, with a class confined to about ten or twelve members. Each day living examples of the condition to be lectured upon should be brought before the student, who would be shown how to examine the case, and have the peculiarities of the disease pointed out to him. In this way he would be able to appreciate the physical conditions when they were lectured upon. Dr Hunter had omitted to refer to examination of the nares, the latest of the specialties, and of very great importance, both in relation to nasal disease and general medicine. Certain forms of asthma and megrim had recently been found commonly associated with nasal disease, the existence of which had been demonstrated by rhinoscopy, and which could be alleviated by local treatment.

Mr A. G. Miller said his attention had been drawn by some statements which fell from Drs Argyll Robertson and Grainger Stewart, in which they insisted on the necessity of making certain subjects compulsory. He held very strong opinions on the subject of adding to compulsory classes. He thought Dr Hunter's paper showed that if the subjects were well taught and of importance, students would attend them of their own accord. Operative surgery was not a compulsory class, and yet it was very well attended. He had for many years felt what the President hinted, that many of their students, or rather their parents, were too poor to be called upon to pay fees for every class which a lecturer might desire to make compulsory. Guinea after guinea was abstracted from them for classes which, if they were not compulsory, they would not attend, and other classes of which they felt the need they were obliged to pass over. He thought it would be better to have a

rearrangement of the classes, and, if possible, a reduction of the fees.

Dr P. A. Young said that when he was a student along with Dr Hunter, they had not the opportunities of becoming acquainted with affections of the special organs in Edinburgh, with the exception of those of the eye. It was not till he went to Vienna that he first saw the membrana tympani. He alluded to this because he thought that by imitating Vienna and New York, in both of which cities there were post-graduate courses, they might get over the difficulty raised by Mr Miller. Their Edinburgh students were as well grounded in the subjects of the ordinary curriculum as were the Vienna students, but they had not the same opportunities of studying the specialties.

Dr Hunter said he had to thank the President and the Society for the very kind reception which had been given to his paper. The reception they had given it left him very little to say in reply. With reference to what had fallen from Dr M'Bride, he wished to remark that when he came to take up the subjects of the ear and throat, rhinoscopy would not be omitted. Dr Young had anticipated him in regard to post-graduate classes, to which he had intended to refer.

Meeting XIII.—July 15, 1885.

Dr David Wilson, *Vice-President, in the Chair.*

I. Original Communications.

1. CASE IN WHICH A LOUD SPLASHING SOUND WAS PRODUCED SYNCHRONOUSLY WITH CARDIAC ACTION.

By William Russell, M.D., M.R.C.P. Ed., Lecturer on Pathology in the School of Medicine, Edinburgh; Physician to the New Town Dispensary, etc.; and J. Cormack Smith, M.B., C.M., Physician to the Cowgate Dispensary.

Mr B., aged 24, a student of medicine, called on Dr Cormack Smith of Abbeyhill on the evening of 14th June 1885, and complained of pain in the precordial region and in the region of the left scapula, not extending below its angle, also of pain in the left arm as far as the elbow. The pain was "numb" in character. In addition, there was a feeling of constriction at the precordia, and inspiration was so painful that only "half breaths" could be taken.

The pain had come on suddenly after dinner, the dinner hour having been 4 P.M. It was so acute that he went and lay down, and he had no sooner done so than the sound, to which we shall have specially to refer, became audible to himself.

The stomach had been out of order for some days previously;

there had been an absence of the normal relish for food, and the bowels had been confined. He had never had any illness of importance, and there was no rheumatic history, and no history of special injury, although he had had a somewhat severe fall from his bicycle a few years before. There is, however, one interesting point in the history: three years ago, during the night, a friend who shared his bed was awakened by a noise evidently proceeding from our patient's inside. Becoming alarmed he roused his friend, and he, with his medical knowledge, referred the sound to his heart, as it was synchronous with its pulsation. It was not accompanied by pain, and in the morning the sound had disappeared. He was quite satisfied that the sound at that time resembled exactly that which we shall presently describe. The only explanation he could offer as a possible cause was that he had walked very rapidly from South Queensferry the previous day.

When Dr Smith first saw him the pulse was 84, and the temperature was slightly raised, but had fallen to normal the following day. He was kept in bed.

On 21st June he was seen by both of us, and we took the following notes of his case:—

The pulse was 64, regular, small and thready, readily compressed and obliterated. There was no albumen in the urine. The tongue was furred, and the stomach somewhat dilated.

When lying on his back a venous pulse was visible above the inner end of the right clavicle, also over the inner end of the left, although less pronounced here than on the right side; there was also supra-sternal pulsation, and pulsation in the upper and outer part of both infra-clavicular regions.

The thorax was well formed. Both sides moved with respiration, the right slightly more so than the left.

The faintest cardiac impulse was perceptible over the precordia.

The hepatic dulness began at the fifth rib. The left nipple was on the fourth rib.

Cardiac dulness extended on the left side in the second space for 1½ in. from midsternum; in the third for 2½ in.; in the fourth for 3½ in. In the third and fourth right spaces it extended for 2½ in. from midsternum. From the fifth to the seventh left rib the percussion note was more resonant than above that, while below this limit the tympanitic note of the stomach was obtained. In the fourth left space, just within the nipple line, a typical crack-pot sound was elicited by sharp percussion, and was invariably accompanied by a sharp pain. Pain was also produced by pressing deeply in this interspace.

On auscultation, there was a distinct friction sound synchronous with the action of the heart, and audible over the entire sternum, also over the area of cardiac dulness in the second, third, and fourth left spaces. In the fifth left space a little within the nipple line, a

friction sound was heard intermittently, and only during inspiration, but occasionally accompanied by a distinct splash. Towards the site of the cardiac apex there was a friction sound present during somewhat deep inspiration, and influenced by the cardiac action. The second sound was accentuated in both the pulmonary and aortic areas.

The foregoing was the result of an examination when the patient lay on his back. He was then turned towards the left side. In this altered position there was visible pulsation in the fourth left space from the nipple line somewhat inwards, and there was at once developed a sound which was so loud that it was audible at the distance of at least six feet from the bedside. The sound was rhythmical, metallic in character, and somewhat splashing or flopping, not unlike the sound produced by water dropping into a cistern. It was synchronous with cardiac systole. During the sound a limited part of the fourth space bulged, and on its subsidence the interspace faintly vibrated. On auscultation over this spot the sound was evidently produced immediately under the stethoscope, and was found to be present during both cardiac systole and diastole, and accompanying it was a friction sound which, however, gave the impression of being distinct from it. The sound, although difficult to describe, was evidently produced by the presence of air and fluid at the site indicated.

On palpation the sound could be felt, and entirely gave the impression of being produced under the hand, and within an area not larger than a florin.

When the patient turned somewhat more on his face the sound disappeared.

It is further to be noted that the patient had the power of inducing, or arresting, or modifying the sound within certain limits, by, apparently, allowing the lower part of the thorax and the diaphragm more or less play. He could also occasionally produce a distinct splash by a rotatory jerk of the body when lying down. The assumption of the erect posture often, indeed as a rule, led to the cessation of the sound.

These are the facts as accurately and as fully as they appeared to us. And the question presents itself, How are they to be explained?

We begin with the hypothesis that the abnormal sound was produced in a cavity containing air and fluid, and that these were set in sufficient motion to produce sound by the shock both of the cardiac systole and the cardiac diastole. But we may go further, and say that this cavity had an opening as evidenced by the crack-pot sound elicited by percussion. The question next arises, Where was this fluid and air? It might be in the pericardium, but there appears to us insuperable difficulties to the acceptance of this view. The effect of change of posture is of moment. The sound was produced when the heart was allowed, by the position occupied, to

come into closer relations with the chest wall; and it must be remembered the sound could be *felt* over a very localized area. Had the air been free in the pericardial sac, the position of the patient on his side would have displaced it in a direction which would have tended to diminish rather than to increase its intensity, if not entirely to obliterate it. Further, to explain the crack-pot sound, it would be necessary to assume an opening between the respiratory organs, or a hollow organ in the abdomen, and the pericardial sac, and that the fluid and air were confined to one part of the sac, that, in fact, it should be encysted, and therefore incapable of displacement by the position of the heart.

Equal difficulties surround the attempt to locate the cavity in the pleura or in the lung. There was an entire absence of evidence of antecedent lung mischief, and beyond the pain on inspiration and the pleural friction there was none at the time he was under observation. He did not even have a cough.

The next question which presents itself is, Was the sound produced by the action of the heart against a dilated stomach? There is no doubt the stomach was dilated, but this will not explain the evident production of the sound at a point readily appreciable by touch, and so evidently localized. It further does not explain the crack-pot sound. To us all of these views are unsatisfactory, if not untenable.

Returning to the hypothesis of a cavity with air and fluid and an opening, we can only explain the phenomena by assuming the presence of a diverticulum from, or a hernia of, the stomach or large intestine through the diaphragm. By granting this, all the facts are readily understood, and the case would present itself thus—an initial gastric catarrh with dilatation, leading to a similar condition of the diverticulum; the retention of air and fluid in it, possibly from a degree of swelling of the mucous membrane at its mouth; while the production of the crack-pot sound by sharp percussion, the finger being pressed deeply and firmly into the interspace covering it, would result from the obstruction at its mouth being still capable of being overcome, although not without causing the patient acute pain. The splashing sound developed by the change of position would thus be due to the heart falling forward against the cavity; its disappearance as the patient turned more on his face would be the result of the obliteration of the cavity by the weight of the heart. The influence of respiration and of certain voluntary muscular efforts would be due to the position of the diaphragm, and therefore of the diverticulum or hernia which it held. The pleural and pericardial friction would be the result of an irritation originating around the cavity, and gradually spreading outwards and upwards from it.

Whether the condition which we have assumed be congenital or acquired, we are not at present prepared to hazard an opinion on. The sound was present for nearly two weeks, and its

cessation was somewhat sudden. We asked Dr Gibson and Dr Bramwell to see the patient, as they are interested in cardiac curiosities; but unfortunately this was delayed too long, for the sound had not been heard for a day or two before their visit, and had not returned.

Dr Bramwell, who had, by the kindness of Dr Russell, had an opportunity of seeing the case, thought they must agree that wherever this sound was produced it was not in the pericardium, lung, or pleura. One peculiarity connected with the case was that when first seen there was pleural, and if he remembered rightly, pericardial friction. This must materially have added to the difficulty of diagnosis. When he saw the patient the condition of heart, lung, and pleura was normal. There was nothing to show that there was any lesion whatever in any of these parts, and this was additional corroborative evidence in support of the view that the sound must have been produced in a cavity outside the pericardium, lung, or pleura. The stomach suggested itself as the most likely place in which such a splashing sound, synchronous with the heart's action, might occur. The fact that the same sound had been heard some time previously, and that without the occurrence of any symptoms of serious disease, was also in favour of the stomach origin of the sound. Dr Bramwell had known a splashing sound synchronous with the cardiac action produced in the healthy stomach. The very definite localization of the sound seemed to have made a great impression on Dr Russell, and the theory which he had suggested to account for it seemed in many respects plausible.

Dr A. Smart said he had listened with great interest to the facts carefully noted in the paper. He did not remember having heard a splashing sound of the nature described, but of the existence of which there could be no doubt in the case referred to. There is a sound which may often be heard, and which, were the whole of the physical conditions present, would be splashing. He referred to a blowing or puffing sound, caused by the action of the heart, and accompanying the systole during full inspiration, and sometimes mistaken for a bruit. The sound was produced simply by the stroke of the heart against the inflated pulmonary tissue. Supposing an abscess or other fluid to exist in the neighbourhood of the heart to which air had access, in such a position as to allow of the heart hitting against it, he could conceive a splashing sound being so formed. He rather leant to that view of the case which would account for the sound by there being fluid and air in some space outside the pericardium, but within the range of the heart's action, and put in motion by it, and so rendered audible.

Dr Russell said he was perfectly familiar with that blowing sound to which Dr Smart had referred, which sometimes accom-

panied the systole of the heart during deep inspiration. The cause of that sound they need not at present discuss. The idea which Dr Smart had formed that the splashing sound in his case was due to an abscess of the lung was entirely untenable, because there were none of the other conditions present that co-exist with abscess of the lung. There were no physical signs or symptoms of pulmonary mischief, apart from pleural friction. Even if an abscess were in the lung, the presence of air in that abscess would be very improbable.

Dr Smart said he had not meant to suggest absolutely that the case was one of abscess of the lung.

Dr James asked where the supposed hernial diverticulum might have occurred.

Dr Russell said it must have been where the sound was heard, opposite the fourth interspace.

Dr James said there were areas where such herniæ could occur, others where they could not. Was this one of the areas where it could occur, or where it could not?

Dr Allan Jamieson asked if there was not a mechanical difficulty in regard to the occurrence of the diverticulum there at all. If it were upwards, as it must be, would not any fluid, which might be the result of a catarrhal condition, gravitate downwards? If this happened they would not have the mixture of air and fluid which was necessary for the production of this sound.

Dr Blair Cunynghame said, might it not have happened, seeing this man was an athlete, that after some active exertion, such as a bicycle ride, the diverticulum may have been nipped by the edges of a hernial ring and a localized catarrh set up, and the swelling of the mucous membrane prevented the passage of air and fluid? Besides, the patient was not improbably in a mortal fright about himself, which would have helped to keep up this condition.

Dr Russell said he had already in his paper considered every suggestion that had been made. The patient was not at all nervous about himself. He looked upon his case as a curiosity, and there was no doubt of the existence of pain when the crack-pot sound was produced by percussion.

2. NUTRITION AND GROWTH IN CONNEXION WITH PULMONARY PHTHISIS.

By Alexander James, M.D., F.R.C.P. Ed., Assistant Physician, Royal Infirmary, Edinburgh; Lecturer on the Institutes of Medicine, Edinburgh School of Medicine.

Of all the causes which lead to the development of the tendency to phthisis pulmonalis, age is unquestionably one of the most important. If we take up a table of the general mortality and one of the proportionate mortality from phthisis alone, we get this markedly ex-

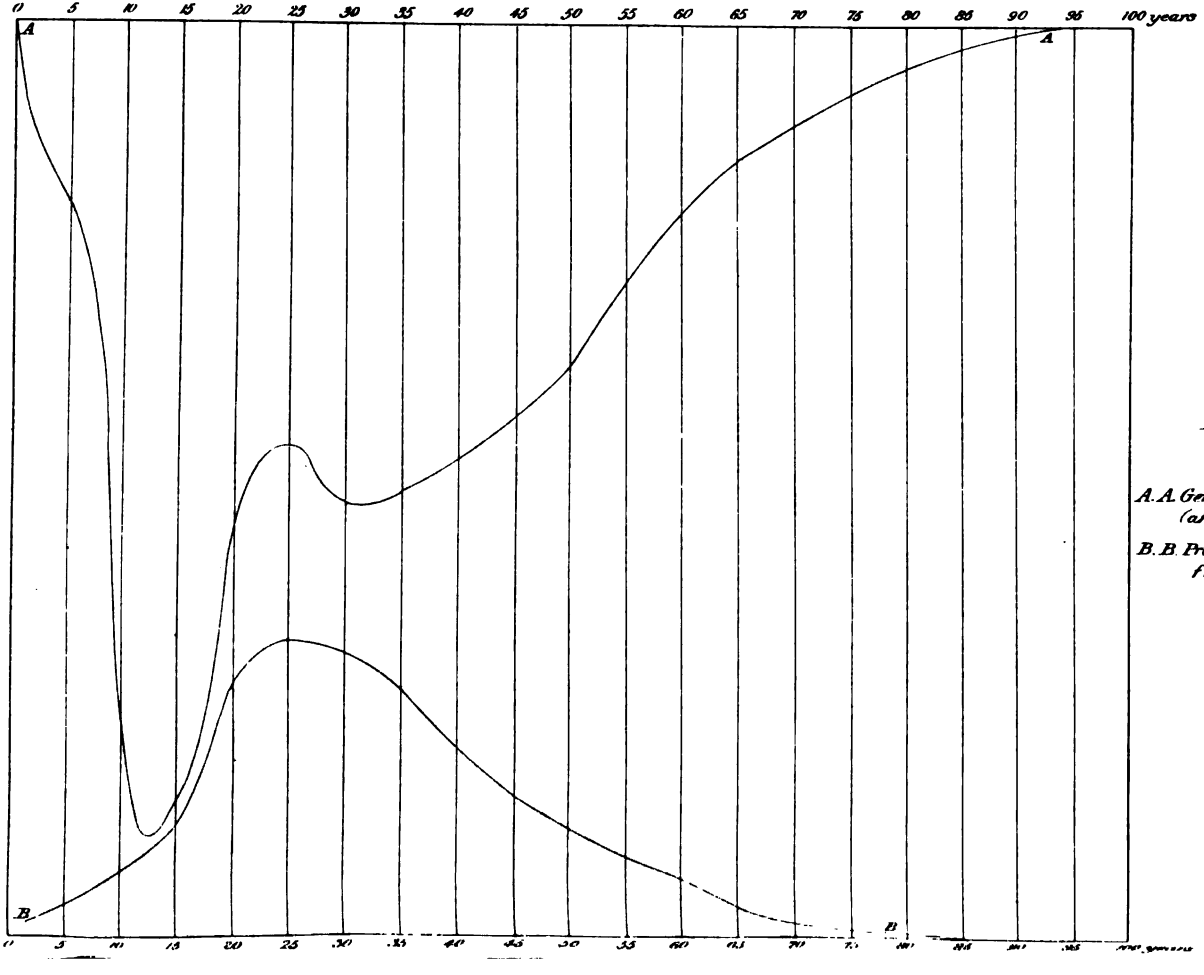
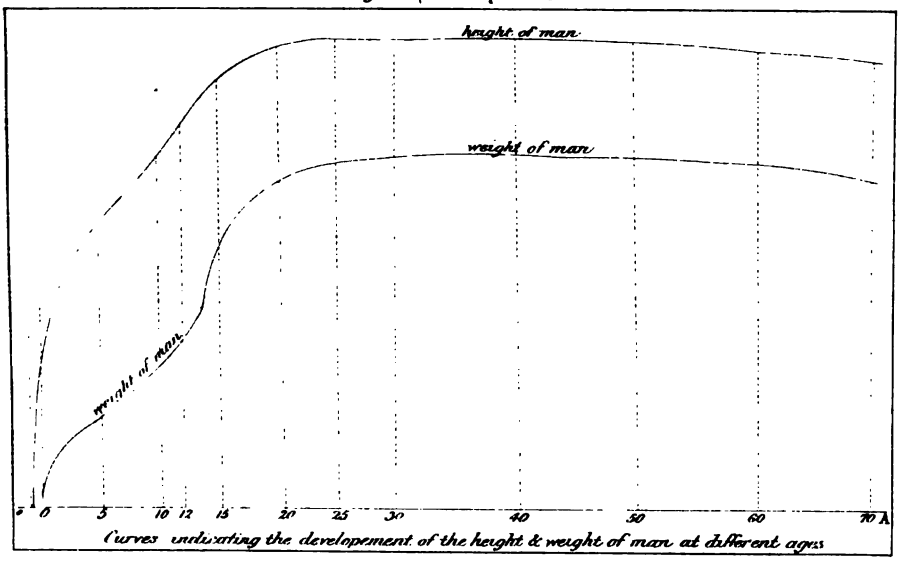


Fig. I.

A. A. General Mortality (after Quetelet.)

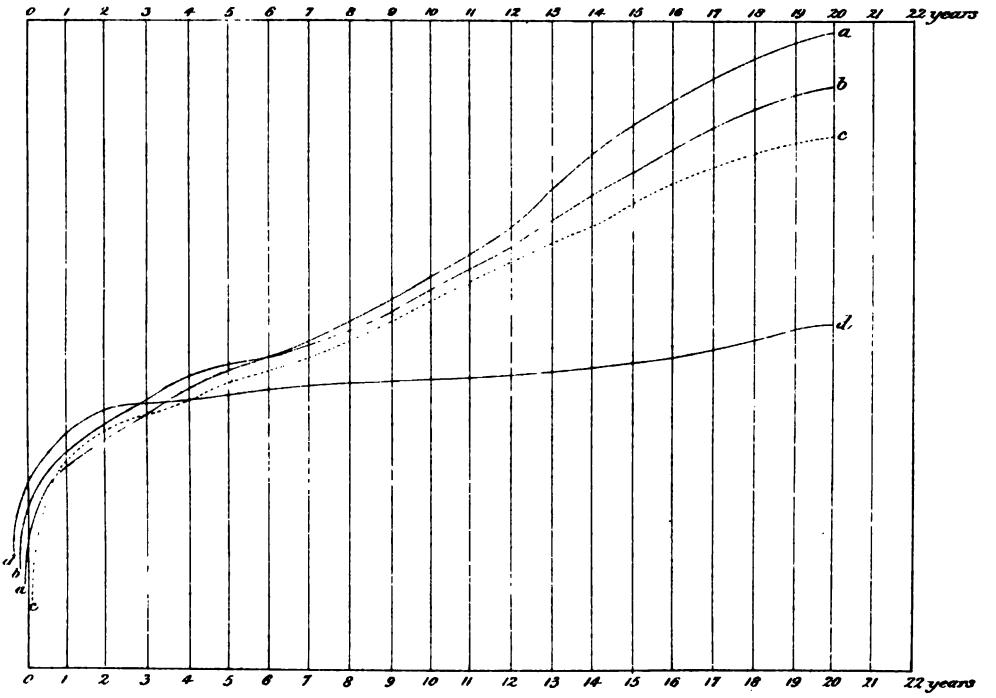
B. B. Proportionate Mortality from Phthisis.

Fig. II (after Quetelet)



Curves indicating the developement of the height & weight of man at different ages

Fig. III.



Curves showing measurements of the circumference of the Thorax - Abdomen & Head at different ages

a Thorax over the nipples. c. Abdomen.
b. do at the 6th rib d Head.

emplified. A glance at the curves (Fig. I.) shows that while the former is excessive at birth, gradually diminishes till about the twelfth or thirteenth year, and then gradually rises to the end, the latter shows during the early years of life an almost complete immunity from this disease, a rapid rise from about fifteen to twenty, a more gradual one to twenty-five, and then a gradual decline.

This latter curve, be it noted, represents the mortality from phthisis relative to the general mortality at different ages, and has been constructed from the Registrar General's returns for Scotland for the ten years from 1870 to 1879, inclusive.[1] If now we constructed a curve to represent the cases of phthisis attending an hospital or dispensary, we should probably find the maximum somewhat later, viz., about thirty years; and if again we constructed a curve to represent the mortality from phthisis at different ages relative to persons living, we should probably find its maximum still later, even up to fifty years.[2] By either of these methods, however, it should be noted that although phthisis is, as has been stated, a disease and a cause of death at all ages, the mortality from it is markedly different from the general mortality.

Whilst the first curve (*a*) illustrates what Addison has described in his "Vision of Mirza," *i.e.*, that the bridge over the tide of eternity has the concealed trap-doors laid thick at the entrance, scarcer towards the middle, and close together towards the extremity; the other curve (*b*) demonstrates that with doors representing risks of death from phthisis pulmonalis alone, a very different distribution would be required to represent what occurs.

And now let us consider fully what the prevalence of phthisis at these ages means. It has been said, and the curve bears this out, that it is a disease which tends to occur at the most vigorous periods of life. What do we mean by the most vigorous period, or in what respects does the period between twenty and fifty differ from those above and below?

If we examine a curve (II.) showing the increase in height and weight of the body, we find that although in it there is a rapid rise about the periods of puberty and adolescence (best marked in weight), there is yet a marked difference between it and the curve of phthisis, in that while the former attains a maximum about the twentieth or twenty-first year, and continues as a straight line, the latter goes on rapidly increasing till about the twenty-fifth or thirtieth year, and then declines. This is important, in that it shows that phthisis is a disease which tends to affect the organism not so much when it is in the act of growing, as after growth has been completed.

But further important information can, I think, be obtained by studying the growth development of the lungs themselves.

[1] For these I have to thank Dr Blair Cunynghame.

[2] Compare Hutchinson, *The Spirometer, Stethoscope, and Scale Balance, etc.*, pp. 43 and 47.

Curve III.,[1] which represents at *a* and *b* the circumference of the chest at the sixth rib and over the nipples, shows a rapid rise after birth and during the early years of life, a more gradual one during boyhood, a more rapid rise during puberty and adolescence, and a maximum at adult age. With this would closely correspond a curve to show the vital capacity at different ages; and although the vital capacity seems to increase slightly until the thirty-fifth year, we may consider that at the age of twenty-one, or thereabouts, the body and respiratory organs have attained their maximum bulk, and the latter their maximum development. That is to say, that whilst in the respiratory mechanism up to the age of twenty-one, or thereabouts, the income has been in excess of the expenditure, the assimilative or vegetative in excess of the disassimilative or animal functions, at this period a balancing of the two is struck, which continues during adult life. Now, although this period certainly represents the so-called most vigorous age, the time during which the greatest production of physical and mental energy can occur, we must remember that, as far as the storing up of energy is concerned, it represents a decline when compared with previous years.

This subject, however, requires closer consideration. Every one will, of course, admit that whilst in early years bodily income exceeds expenditure, in adult life the two are balanced in health, so that during this period the weight remains the same from day to day. The question, however, is, How is this balance struck? It might be by one or other or both of two ways,—by increase in the expenditure, by diminution of the income, or by both. The first of these will perhaps occur to some as being most likely. The fact that the real work of life, be it physical or mental, is only begun after growth is complete, may lead us to suppose that in adult life the income remains the same, but that that energy which formerly went to increase the bulk and weight of the body is now expended in physical or mental work. But a closer consideration of the subject shows us that the increased capability for work during adult life is rather a question of development, is the result of our being then better able to direct energy. Although the total amount of carbonic acid and urea excreted daily is greater in adult life than in childhood, the amount relative to body weight is very much less, and shows that the molecular and chemical changes upon which life depends take place during adult years with greatly diminished activity.[2] But now, can the balancing be better explained by diminution of income? Scientific observation bears out what all have daily observed, that as animals advance from early to adult life, the amount of food required undergoes a great diminution relative to body weight,[3] and that this should be the case is evident from what we

[1] Constructed from the measurements given in Table VI.

[2] Vierordt, "Physiologie des Kindesalters," in *Gerhardt's Handbuch der Kinderkrankheiten*, pp. 353–370 and 379.

[3] Vierordt, *ibid.*, pp. 403 and 413.

have noted as regards the greater relative expenditure of early years. In adult life, therefore, there is relatively less assimilative power and relatively less expenditure as compared to childhood, although *absolutely* both are increased. As to how the balance is struck, however, we cannot speak definitely, owing to want of sufficient data, but still some important conclusions can be drawn. Seeing that in both income and expenditure there is a gradual diminution relative to body weight as the animal passes from early to adult life, and remembering that in early life the relatively great expenditure is not only completely replaced by the income, but is actually exceeded in order to admit of growth, we must admit that in the striking of the balance of adult life the diminution of the income is the more important item. Although the absolute amount of income and expenditure is greater in adult life than in childhood, we must remember that this relative diminution of income means a lessened formative power, and probably no better example of this could be obtained than the rapid renewal of parts in certain young animals. As is well known, such animals as newts, etc., have the power of reforming limbs, tails, etc., after amputation when young, a power which rapidly diminishes as age advances. Of course, between early life on the one hand and adult life on the other there is no hard and fast line; but whilst we can recognise in the transition an increase in the absolute and diminution in the relative amounts of both income and expenditure, we must recognise also a greater relative diminution in the income, a greater relative diminution in the vegetative functions of the body.

But these considerations are important in connexion not only with the work of the body, but with the heat, and to this I would now direct attention. Heat, as we know, is constantly being produced, and we know also, as constantly being lost, mainly in man by radiation, conduction, and evaporation from the skin. A matter of interest, therefore, in connexion with our subject is to know the extent of surface of skin at different ages, inasmuch as from this we may be able to deduce some information as to the relative loss of energy to the body in the form of heat. Tables I. and II. show that the extent of skin surface to body weight is greatest at birth, and gradually diminishes as growth goes on, becoming least at and during adult life. Thus, then, although we must not lose sight of the fact that the loss of heat from the surface of the body can be modified by clothing, etc., we may say that as age advances less and less energy in the form of heat in proportion to body weight will be lost to the body from the skin. Taken into consideration with the fact that $\frac{4}{5}$ of the total energy of the body is expended as heat, and of this the great proportion in man by the skin, this gradual diminution in extent of skin surface in proportion to weight is specially interesting, as it shows that as years advance there occurs less and less requirement for heat production, *i.e.*, for

expenditure. With this it is also specially interesting to notice that the apparatus for bringing about expenditure, *i.e.*, the lungs, become proportionally more extensive as age advances. As Vierordt has stated, the respiratory capacity in growing years increases more rapidly than the weight; and although that observer notes that the proportion of the respiratory capacity to body weight may not differ much at different ages,[1] the proportion of the respiratory capacity to extent of skin surface seems to become greater as age and size increase.[2] To this point we shall advert later on.[3] Meanwhile we must observe that the fact that with the much greater relative loss of heat which the large extent of surface in the young animal will entail, there is associated growth or increase in bulk, and the manifestations of physical and mental energy, great in proportion to the bulk of muscle or brain, is to be looked upon as further evidence that the period of adult age is, as compared with that which precedes it, one of decline in the vital assimilative power. Bearing further on this point, and of great practical importance in connexion with pulmonary phthisis, there are certain further considerations which I propose now to consider in detail. These are that at the onset of adult life, *i.e.*, of the so-called most vigorous period of life, certain important physiological and pathological phenomena are to be observed. Those which I propose to discuss are mainly three in number, viz., the development of the reproductive functions, the disappearance in scrofulous subjects of enlarged cervical lymphatic glands, and the growth of hair.

I. *Development of the Reproductive Functions.*—The connexion between innutrition and reproduction is well established, and the fact of the development of the reproductive functions about puberty, when, as we have seen, the excess of income over expenditure ceases, of course bears this out. In this connexion the great increase in the vital capacity about this period is interesting, as showing arrangements for promoting expenditure; and I would also here suggest that the enlargement of the respiratory passage, which leads in the male to the cracking of the voice, may have a similar explanation. Its association with the growth and development of the testicles is noteworthy; and I would here also point out that the fattening which occurs after removal of the reproductive organs in animals shows also the connexion between these functions and tissue oxidation.

[1] See also Table II., column 10.

[2] See Table II., column 9.

[3] This fact that the respiratory capacity bears an inverse proportion to the extent of skin surface may, of course, mean that whilst skin and lungs are together concerned in giving off heat, the proportionate amounts given off by such differs at different ages and in individuals of different heights. It is important, however, to observe that oxygen does not appear to be so necessary for assimilation. This can be concluded from the fact that new-born animals have a great power of resisting asphyxia, it being in intrauterine life that assimilation is most active.

But these points are of practical importance in connexion with phthisis. How menstruation, lactation, and pregnancy affect this disease I do not propose at present to discuss, but the relationship between reproduction and phthisis generally is interesting. Pollock (*Elements of Prognosis in Consumption*, p. 345), after discussing the injurious effects of marriages of consanguinity, says: "This question of large families is also of interest. Parents weakly from any inherited or personal disease are frequently very prolific, and become the propagators of constitutional and organic disorders. We have known as many as seventeen brothers and sisters die of consumption. As a matter of experience, there is no doubt that the phthisical are prolific, and endless instances occur of a mother conceiving when in an advanced stage of tubercular disease, and having a child which survives only a few months; nor is the generative power confined to the mother. Men in much advanced phthisis often marry and have children." Again, "Intermarriages of individuals remote from each other in ancestry, in social condition, in constitution, and in what is called temperament, but above all the unions of persons of different race and country (if direct taint of disease be absent on both sides), are very generally free from resulting affections of constitutional origin in their children. It is perhaps true that marriages between individuals of different races are not very prolific; and if this be the case, it is another reason for the greater vigour of their offspring, for the members of large families are not generally so individually strong as those of small ones."

This question of large families in connexion with the tendency to disease is interesting, and I regret that I have at present no statistical information to offer in connexion with it. I give, however, the above quotation as an expression of opinion, and I think that it will be shared in by most observers. But, on the other hand, deficient health in individuals is often associated with sterility; and this requires notice, as it might appear to be an argument against what has been above stated. I think, however, it is not, because we have examples of the close connexion of the two, as opposed to healthy nutrition and reproduction; *e.g.*, Matthews Duncan pointed out that in women, as age advanced, barrenness and twinning were frequently associated.

If one may generalize, one would say that in the present state of evolution a certain number of offspring is normal, whilst a number, markedly above or markedly below this, and, of course, sterility, are equally to be associated with a disturbance of the normal balance between assimilation and disassimilation on the part of the individuals.

II. *Enlarged External Lymphatic Glands.*—The connexion in scrofulous subjects of enlarged lymphatic glands with pulmonary phthisis is well known, and recognised to such an extent that it has been stated (Ziemssen's *Cyclopædia*, v. p. 641) that a scrofu-

lous gland is tubercular, and the extirpation of such as a preventive to the absorption of tubercular matter, and consequent development of phthisis by auto-infection, has been proposed (Ziemssen, *ibid.* 605–642). But all of us must have observed the infrequency of enlarged cervical glands and pulmonary phthisis in the same subject, and a little consideration will, I think, explain this.[1] It shows that enlarged glands are associated with an earlier period of life than the phthisical—that, in fact, they are associated with the period when daily income is in excess of expenditure. As soon, however, as the income and expenditure of the organism are balanced, as they are when growth ceases and the respiratory functions reach a maximum, these glands diminish or disappear. This seems to be simply the result of the fact that absorption then becomes more active, and the disappearance of these glands as the result of counter-irritation; or, as is frequently noticed on the supervention of a fever, an acute pneumonia or even of phthisis itself can be ascribed to this cause, and demonstrates the association of the period of adult life with an increased expenditure relative to income of bodily energy. Here, again, I think some further suggestions may be made,—*e.g.*, looking upon those enlarged glands as tubercular foci, the supervention of tubercular at an earlier age than ordinary phthisis, may have an explanation in the fact that coincident with the disappearance of these glands there will be an increased risk of auto-infection with tubercular matter. Again, the risk of phthisis developing about that age in a scrofulous individual, as the result of his or her being somewhat out of health and feverish, can be readily understood by remembering that then resorption is very likely to occur.

III.—The growth of hair at different parts of the body, which supervenes at puberty, and which, in connexion with the trunk and limbs at all events, increases in amount as age advances, is another consideration of importance in connexion with this period of life. Hair, like every other epidermal appendage, is for use, as every evolutionist will admit; and one important use is to diminish the loss of heat from the surface of the body by radiation and conduction, etc. Now, although as the result of artificial conditions the growth of hair in man is comparatively rudimentary, we may suppose that its absence from the body before puberty and adolescence, and its presence after, seems to denote that then, *i.e.*, during adult life and old age, there is a proportionate innutrition, for it shows that the preservation of the heat of the body by external mechanism is rendered at these periods of consideration. I am aware that viewing the growth of hair as evidence of innutrition in this way may readily be questioned, and, indeed, may be denied or even contradicted; *e.g.*, it may be stated that the presence of hair on the chest and limbs is found not in women but in men, and, indeed, is found in the most muscular and powerful

[1] Pollock, *ibid.*, p. 90.

representatives of the sex; that although it is true that as age advances the hair of the body, limbs, etc., may increase, yet that of the head diminishes, and that the distribution of hair over the surface and at the various orifices of the body requires explanation. All these objections may, I think, be explained. The hairy chest and arms of the out-door labourer, sailor, etc., is, I would submit, due to the fact that in such these parts are more or less habitually exposed to cold, wet, etc., and that although the individuals are in good health and possess great muscular vigour, there will be *at these parts* a proportionately great loss of heat.

The bald head of age may, I think, be explained by remembering that hair, like every other living thing, has its periods of growth, development, and decay; and that as in man the scalp is the locality at which hair is first grown, from there also will it first disappear. The distribution of hair over the surface of the body, and its presence at the various orifices, now requires explanation.

Here I would remark that what I propose is somewhat speculative; still it is, I think, of sufficient value to warrant its being brought forward. It is that such apparent anomalies may be explained when considered in connexion with temperature sensations. As every physiologist knows, the localities most sensitive to temperature are not the same as those most sensitive to touch; *e.g.*, as regards temperature, the back of the hand is more sensitive than the palm, and the lips, cheeks, neck, and ventral aspect of the trunk are specially sensitive. These are the localities where hair is found; and the presence of hair at the orifices of the body has, I believe, a similar explanation. To understand this latter, however, let me call to your recollection the following considerations:—The sensation produced by any stimulus touching the skin is intensified by comparison with a part not touched; *e.g.*, if we dip one finger in mercury we, of course, experience a sensation of pressure, but this is felt more markedly at the surface of the mercury; and if we move the finger up and down in the mercury, we feel as if we were moving the finger up and down through a ring. If, now, the mercury be cooled or heated, the sensation of a ring of cold or heat is produced as the result of the intensifying of the sensation by comparison with a part not stimulated. Now, at the various orifices of the body we have a similar condition in the transition of a surface (the skin) exposed to external temperature, with a surface (the mucous membrane) protected from such.

These considerations, therefore, seem to me not to invalidate the assertion that the growth of hair means an effort on the part of the organism to preserve it from loss of energy in the form of heat; and that its somewhat sudden appearance about puberty and adolescence, and more gradual increase in after years, means a somewhat sudden and afterwards more gradual diminution in what we may call the vital energy. Of course the hair has many

other uses, and doubtless a most important one is, as Paget[1] says, that "it removes from the blood the various constituents of its substance, which are thus excreted from the body;" and, he goes on to say, "this excreting office appears in some instances to be the only purpose which the hair serves; as, for example, in the fœtus. Thus in the fœtus of seals, that take the water as soon as they are born, and, I believe, in those of many other mammals, though they are removed from all those conditions against which hair protects, yet a perfect coat of hair is found within the uterus; and before or very shortly after birth this is shed, and is replaced by another coat of wholly different colour, the growth of which began within the uterus. . . . . The lanugo of the human fœtus is an homologous production," etc. That hair, therefore, acts to some extent as an excretion, is no doubt the case; but its value in economizing the heat of the body is quite as important, and in the case of many animals, of course, more so.

And now for the practical application of these remarks. All have observed the luxuriance of the hair of the scalp and beard, the long eyelashes and the frequent development of downy hair on the posterior part of the neck and back in early phthisis, and in individuals in whom a tendency only to this disease exists. We are aware that in the course and later stages of the disease a premature wasting of the hair of the scalp frequently occurs; but the apparent contradiction is got over by supposing that in these cases the hair follicles have atrophied as the result of the disease, just as they are apt to do after an ordinary fever. I am well aware that in connexion with the growth of hair many other circumstances have to be taken into consideration,—selection, natural and artificial, heredity, effects of clothing, etc.; and I am also aware that the explanations of its presence and absence which I have been offering are mainly speculative. I submit them, however, as data for a working hypothesis; and I am the more inclined to do so, inasmuch as by many an excess of hair is looked upon in the main as a sign of strength. I have no hesitation in stating that my experience of disease has led rather to the opposite conclusion.

I would therefore suggest that the growth of hair, like the development of the reproductive functions and the disappearance of enlarged cervical glands, is also to be looked upon as evidencing that the adult period of life is, as compared with previous years, one in which there is a relative decline in the vital assimilative energy.[2]

[1] *Lectures on Surgical Pathology*, p. 19.

[2] Fat adults are, as a rule, less hirsute than thin; and here for an explanation one may suppose that in such the assimilative power is markedly developed, and probably also that the subcutaneous fat will act as a sufficient protection to loss of heat. This may to some extent explain the difference between men and women as regards the amount of hair on the body and limbs.

But now we have been considering only the one-half of our subject, *i.e.*, having seen that (see Curve 2, Fig. I.) phthisis begins to become prevalent about puberty and reaches a maximum about the middle of adult life, we have been considering in what respects these years differ from the previous. A glance at the curve shows, however, that as age advances the tendency to phthisis gradually diminishes. Can we now get any information as to how the later years of life differ from those of maturity? Seeing that in early years and in old age there is a comparative immunity from phthisis, can we recognise any similarity between these two as distinguishing them from the period of maturity? The first supposition which is likely to occur is probably a contradictory one to this, viz., that if maturity differs from early life, old age must differ still more. Let us, however, examine the matter more closely. We have seen that the period of maturity differs from that of the preceding years in the relative diminution of income, and as aided, perhaps, by the increase in the respiratory capacity by an (relative to income) increased expenditure. Let us now apply these considerations to the period of old age.

As age advances the respiratory capacity diminishes; as has been shown, it diminishes after 35 at the rate of about 1½ cubic cm. per year. The cause of this we may suppose to be the gradual increase in the size of the pulmonary air cells, the occurrence, in fact, of an emphysema, which, although the lung is voluminous, renders less the quantity capable of being displaced.[1] This indicates a diminished expenditure of energy, and is associated with a diminution in the capability for physical and mental work. The extent of surface of the body, however, remaining the same, there is practically no difference in the amount of heat lost; owing, however, to the lessened production of heat by movement, etc., artificial means are used to maintain it. In this way, as age advances, there is in effect a diminution of expenditure, and to such an extent does this frequently occur, that a balance of a kind to the good takes place, indicated by the reappearance of fat.[2] With this lessened waste is further associated a diminution in the reproductive power as indicated by the diminishing fecundity of marriages as age advances. Further, we must remember that the lessened respiratory capacity in old age, to which we have referred, is frequently very considerable, a certain amount of emphysema being almost normal after 50. Hence, although the expenditure is diminished, the lungs themselves must, in many cases, function actively, a condition analogous to that found in childhood, when, with a comparatively small vital capacity, there is proportionately great expenditure. In this way, then, old age has similarities to the early periods

[1] We must remember that the size of the air cells will greatly influence respiratory activity. The small air cells of the child will allow of a large surface of blood and air being brought into contact.

[2] Moderate accumulation of fat is normal in people over 50. Traube quoted by Ebstein, on *Corpulence*, p. 5.

of life, but, of course, there are many important distinctions,—the main one being that, whilst in early life the balance to the good is the result of excess of income, in old age the reverse is the case. We must remember, also, that in old age the assimilative power is also becoming impaired, and that the amount of this as compared with the expenditure varies greatly. In most individuals it is as we have stated, for a time at any rate, relatively in excess of waste, leading to fat accumulation. But such is not always the case. We find cases, on the one hand, in which the assimilative power relative to the expenditure diminishes rapidly as age advances, leading to feebleness, degeneration, and premature dissolution, whilst, on the other, these two processes may diminish *pari passu*, and very gradually leading to an active, and, as regards nutrition and reproduction, energetic old age. Of course these three varieties of the appearances presented by the decline of life are not always so clearly distinct as perhaps our description would lead us to expect, but that they indicate realities, every one will, I think, allow. Now it will be noticed that the first variety mostly resembles the period of youth, the third less so, and the second least of all.[1] The application of such considerations to phthisis is obvious,—in the first there is almost complete immunity from this disease, a state of affairs which cannot be said of the third, and still less of the second.

A general view, then, of the stages of youth, maturity, and decline in connexion with pulmonary phthisis leads us to connect that disease with the second of these, with the period when the income is just sufficient to meet the daily expenditure, *i.e.*, has (relative to the expenditure) sustained a diminution. This statement requires qualification. It may be argued, if pulmonary phthisis is apt to supervene at this period it should be apt to occur in any case where the quantity of food is insufficient. But does it? I think not. As has been remarked, "Good food and bad air are more hurtful than bad food and good air," and in cases of long standing inanition, as from œsophageal stenosis or in famine districts, pulmonary phthisis is rare.[2] How can we explain this? Simply by stating that what we now mean by assimilation is the vital power of transforming the nutritive material derived from the food into tissue, a power which the individual is born with, which, with greater or less variations, gradually diminishes as age goes on, and which is not necessarily interfered with in cases of inanition such as we have referred to. Pulmonary phthisis is apt to supervene when this power is diminished in vigour; at the periods when the reverse is the case there is comparative immunity.

But, now, if phthisis is common at this period we should expect to find, then, a prevalence of other diseases. Into the connexion of

[1] Compare Paget, *Surgical Pathology*, p. 82.

[2] This of course has been denied, but compare Ziemssen's *Cyclopædia*, vol. v. p. 488.

phthisis with such I shall not at present enter, but there is one closely allied to phthisis which we may now appropriately discuss, viz., tubercle.

Tubercle occurs in man, affecting different organs and tissues—the brain and intestines, as well as the lung—and it is apt to affect those at different ages,—in the case of the brain during infancy, and intestines during boyhood or girlhood. How does the view that pulmonary tubercle is apt to develop when assimilation has, relative to disassimilation, diminished, tally with the fact that it attacks those other localities in earlier years when increase in weight and bulk is rapidly taking place? Premising that pulmonary phthisis does not necessarily mean tubercle, we shall nevertheless find, I think, that the occurrence of tubercle during the early years of life is after all not conditioned differently from its occurrence in the lung.

Let us first take brain tubercle, the so-called tubercle of infancy and childhood. Examination of the Table III., representing Rilliet and Barthez's 98 cases, shows it to be relatively uncommon in early infancy, but rising rapidly, to reach a maximum between the fourth and fifth year, and then gradually to decline. Undoubtedly, then, it may be said brain tubercle occurs at periods of energetic growth, but this does not invalidate our conclusion, for we have now to do with the brain growth, and the period of maximum growth of the brain is very different from that of other parts of the body.

Curve *d*, Fig. III. has been constructed from measurements of the heads of 500 individuals (males) between birth and the 20th year, and it shows that the greatest growth of the brain takes place in the first months of life, and that after the second or third year the growth is comparatively very slow. Brain tubercle, therefore, cannot any more than pulmonary be associated with a period of life when assimilation is in excess.

Next, as regards mesenteric tubercle, the so-called tubercle of boyhood. Table IV., taken from Rilliet and Barthez's 215 cases, shows that it reaches its maximum between the sixth and tenth years, and these observers have stated that it is rare before the fourth year. By some observers it is said to occur earlier, and Widerhofer in Gerhardt's work makes its maximum between the fourth and fifth year. That abdominal tubercle should occur early and be somewhat widely scattered over the early years of life, we can understand for many reasons, more especially, perhaps, because then the risk of irritation from errors of diet is at its maximum, but there is, I think, no doubt that true abdominal phthisis is to be associated with a period of life later than that at which brain tubercle is apt to occur. All of us have had many cases of it during puberty and adolescence, which cannot be said of tubercular meningitis.

To ascertain at what periods of life the digestive organs present

their most active growth, various methods might be made use of; *e.g.*, we could get information by measuring the length, etc., of the stomach and intestinal canal at different ages from birth onwards. Or the proportionate size of the abdominal cavity at different ages can be made to afford data, and of this method the following from Liharzik, quoted by Vierordt in Gerhardt's work,[1] is an example—

	Birth.	21st Month.	7½ Yr.	Adult.
Belly, . .	100	160	240	260

This shows growth comparatively slow after the seventh year.

In order to obtain further information on this point, let us look again at Fig. III., curves *a*, *b*, and *c*, which show measurements of the circumference of the body at the sixth rib, over the nipples, and at the umbilicus. These measurements were made in order to ascertain the relationship between the digestive and respiratory functions at different ages. The curve shows that at birth the circumference at the sixth rib is greater than at the nipples, that about the fourth year they are equal, that afterwards the upper circumference of the chest proportionately increases, and that the circumference at the umbilicus, apparently greater than that at the nipples about the first or second year, does not increase to anything like the same extent at puberty and adolescence. All this seems to show that after the fourth or fifth year the growth of the digestive organs is exceeded in activity by that of the respiratory. The occurrence, therefore, of abdominal tubercles is, I believe, in accordance with the rule, that tubercle tends to develop in a tissue after the period of maximum growth, *i.e.*, when assimilative power begins to fail.

I am indebted further to Drs MacBride and Caird for pointing out to me that a similar law may be applied as regards tubercle of the larynx and testes. In both these organs it occurs most frequently some time after puberty (about the 22nd year), *i.e.*, after the period of maximum growth.

But closely connected with our subject, and of great practical importance in connexion with phthisis, is the *height* of the body. Remembering that, as Hutchison has pointed out, the vital capacity increases in a direct ratio with the height, and that, as we have seen, phthisis prevails when the vital capacity is at its greatest, the influence of stature becomes peculiarly interesting. In 1586 cases (adult males) admitted into the Royal Infirmary, of which 237 were phthisis, I have obtained records of the height, and Table V. shows the result. Of the phthisical cases, the greatest number (16·8 per cent.) were in height between 5 ft. 8 in. and 5 ft. 9 in., and 6·3 per cent. of these were over 6 ft. in height. Of the cases other than phthisis, the greatest number

[1] *Handbuch der Kinderkrankheiten*, vol. i. p. 272.

(18·5 per cent.) were between 5 ft. 6 in. and 5 ft. 7 in., and 4·2 per cent. were above 6 ft. I regret that the number of cases out of which this table has been made is so small, but I think that although phthisis is found at all statures, every one doing clinical work must have observed its prevalence among individuals above the average height. I have been informed that many of the publicly exhibited giants fall victims to phthisis, and that in regiments of guards the proportion of phthisis is somewhat above that of ordinary regiments. But why should this be? Several suggestions might be put forward in explanation; the only one, however, which I shall mention now is that which can be obtained by considering together the vital capacity and the surface extent of the body. Remembering that for nutrition, function of the part is necessary, one may suppose that as in a tall individual the extent of surface of body in proportion to weight is less than in a short one, there will be less proportionate loss of heat, and hence the respiratory activity need not be so active. The lungs, therefore, will not have such a demand made upon them, and will function less actively. But further, as we have seen in tall individuals the lungs are specially large, the vital capacity being great, hence a second risk of deficient functional activity will occur.[1] The fact has again and again been observed, that although the weakly phthisical build is characteristic, we frequently find phthisis developing in individuals tall and of extra good physique. The explanation of this, I think, is obvious. Such individuals are fitted for an avocation requiring plenty of physical work in the open air; give them this, and they remain healthy. Give them, on the other hand, indoor town life and sedentary work, they readily succumb to phthisis. We must look upon this, I think, simply as one of the many sacrifices which nature requires in her efforts to transform the struggle for existence from the physical to the intellectual.

But from the occurrence of phthisis preferably in tall individuals another deduction can, I think, be drawn, and that is an explanation of the view put forward by Laennec and many others, that phthisis does not, to a great extent, supervene as the result of "catching cold." While acknowledging the pernicious effect which such a theory would have if accepted and acted upon, I would submit that for this, as for all such extreme views, there is a certain foundation in fact. The evil effects of a chill on the body must, *ceteris paribus*, be most marked in those cases in which the surface extent bears the greatest proportion to the body weight, *i.e.*, in short individuals and in children, less in tall adults. The fact, however, that it is rather in the latter that phthisis is met with may be looked upon as denoting that it is something else than simple loss of heat which is the main determining cause of this disease.

Conclusions.—1. That phthisis tends to occur when the assimilative

[1] See Table II., columns 8 and 9.

power fails, as indicated by the occurrence of it, or tubercle in the lungs, intestine, and brain at different ages, and that the development of the reproductive function, the disappearance of enlarged cervical lymphatic glands, and the growth of hair, indicate a lessened activity in the vital processes in adult life as compared with early years.

2. On the general principle of the connexion between supply and demand, we may suppose that this assimilative power is to a greater or less extent dependent on functional activity of the part. This seems borne out by the fact, that in tall people with large lungs and with proportionately less demand for vital activity (*i.e.*, less loss of heat) phthisis is common, and also by the fact that as age advances the natural tendency to emphysema, by increasing the functional activity of the lungs, seems to render them less liable to phthisis.

3. This assimilative power, though in part dependent on functional activity, is *innate* as regards the individual. Of this we have evidence in the different sizes to which individuals grow, the functional activity being the same, and in the varying proneness to phthisis in individuals, the surroundings being the same.

TABLE I.—*Proportion of Skin Surface to Body Weight at different Ages,* □ *cm. to* 1 *kilog.*

6 days, ...	=829.
6½ months, ...	=624.
6 years 8½ months, . .	=458.
14 years, ...	=354.
25 years, ...	=301.

—Meeh, quoted in Vierordt, *ibid.*, p. 288.

TABLE II.—*Proportion of Skin Surface to Body Weight and Vital Capacity, and of Body Weight to Vital Capacity, in* 6 *Individuals of different Ages and Statures.*

		Age.	Height.	Weight.	Skin Surface.	Vital Capacity.	Ratio—Weight to Skin Surface.	Ratio—Vital Capacity to Skin Surface.	Ratio—Weight to Vital Capacity.
		yrs.	ft. in.	st. lbs.	sq. in.	c. in.	lb. to sq. in.	c. in. to sq. in.	lb. to c. in.
1	Boy	7	3·6	3·8	1353	77·6	1 : 27·06	1 : 17·4	1 : 1·54
2	do.	11	4·3	5·4	1728	91·8	1 : 23·35	1 : 18·8 [1]	1 : 1·22
3	do.	15	5·0½	7·10	2398	148·4	1 : 22·2	1 : 16·1	1 : 1·37
4	Man	28	5·5½	9·12	2768	216·0	1 : 20·07	1 : 12·8	1 : 1·59
5	do.	28	5·9¾	11·4	3024	247·0	1 : 19·1	1 : 12·2	1 : 1·56
6	do.	24	6·2½	15·5	3556	327·0	1 : 16·4	1 : 10·8	1 : 1·52

[1] This exception is probably due to the fact of the vital capacity being proportionately lower than in the cases of the other two boys.

TABLE III.—*Cases of Tubercular Meningitis.* Rilliet and Barthez, vol. iii. p. 511.

	Paris.	Geneva.	Totals.
5 months,	0	1	1
9 „	0	1	1
1 yr. to 2½ yrs.,	4	13	17
3 „ 5½ „	11	23	34
6 „ 7½ „	7	16	23
8 „ 10 „	10	5	15
11 „ 15 „	4	3	7
			98

TABLE IV.—*Case of Abdominal Tubercle.* Rilliet and Barthez, vol. iii. p. 841.

	Age.				
	1 yr. to 1½ yrs.	3 yrs. to 5½ yrs.	6 yrs. to 10½ yrs.	11 yrs. to 15½ yrs.	Totals.
Stomach, . .	4	11	5	1	21
Small Intestine,	25	34	49	26	134
Large Intestine,	9	17	21	13	60
Totals, . .	38	62	75	40	215

TABLE V.—*Stature of Patients admitted into the Royal Infirmary,* (1586 *male adults*).

Ft. in.	Phthisis (237 cases). Per cent.	All other Diseases (1349 cases). Per cent.
5 2 and under,	2·1	3·0
5 3	1·6	2·3
5 4	5·9	6·5
5 5	8·8	10·9
5 6	11·8	18·5
5 7	13·9	17·0
5 8	16·8	13·1
5 9	14·7	10·2
5 10	12·6	8·4
5 11	5·0	4·0
6 0 and over,	6·3	4·2

For data for these results I have to thank Professors Grainger Stewart and Fraser, and Drs Muirhead, Brakenridge, and Wyllie.

TABLE VI.—*Measurements of the Circumference of the Thorax, Abdomen, and Head at different Ages* (500 *cases, males.*)

	Thorax.		Abdominal. Umbilicus.	Head.
	Over Nipples.	Over 6th Rib.		
	in.	in.	in.	in.
Birth	12·7	12·9	12·0	13·6
6 mos.	16·0	16·7	16·0	17·5
1 yr.	17·7	18·3	17·0	18·4
2 „	18·5	18·7	18·4	19·2
3 „	19·8	20·1	19·2	19·7
4 „	20·3	20·7	19·4	19·6
5 „	21·0	21·0	20·6	19·7
6 „	21·4	21·5	20·7	19·8
7 „	21·9	21·8	21·0	20·0
8 „	22·6	22·0	22·0	20·0
9 „	23·5	22·9	22·7	20·1
10 „	24·2	23·4	23·7	20·2
11 „	25·3	24·2	24·1	20·5
12 „	26·0	25·0	24·5	20·6
13 „	27·6	26·2	25·4	20·6
14 „	28·6	27·0	25·6	20·8
15 „	29·9	28·1	26·9	20·9
16 „	30·8	29·0	27·5	21·1
17 „	31·6	29·7	28·0	21·5
18 „	31·4	30·5	28·5	21·7
19 „	33·0	31·0	29·2	22·0
20 „	33·7	31·5	29·5	22·2

The Chairman said he was sure he expressed the sentiments of the Society when he said they had listened with much pleasure to this very valuable paper by Dr James, even though they might not agree with all the conclusions he had come to, especially the view that the growth of hair was a sign of physical weakness.

Dr Blair Cunynghame said he could not allow such a paper as this to go without expressing commendation of it. It was one of the most admirably-reasoned papers he had heard read in the Society for a long time, and he was only sorry there were so few present to listen to it. The point bearing upon the tendency of young people, who had once suffered from enlarged glands but had recovered from them, to attacks of phthisis was important, and very particularly so in reference to insurance; and in examining young people who had recovered from enlarged glands, it should be borne in mind their liability to undergo other physical degenerations was great. He was not altogether inclined to agree with Dr

James in his view as to the growth of hair indicating physical weakness. The growth of hair on the arm might be due to local irritation from exposure to the sun and air. It was a well-known fact that in parts affected by chronic inflammation, as periostitis, or in the neighbourhood of old ulcers, for example, an overgrowth of hair occurred, and it was not uncommon to apply cantharides in baldness to induce a growth of hair.

Dr Allan Jamieson said one of the points that struck him in Dr James's admirable paper was the reference to the evolution of the reproductive organs at a time when there was an increase of expenditure and diminution of income. It was quite true that the reproductive organs in the female might be said to be accompanied by an increase of expenditure at puberty, but it did not seem to him that it was the same in the male. In our country, at least, the expenditure from these organs in the male did not occur till some time later in life. With regard to the occurrence and disappearance of enlarged glands at a period antecedent to the occurrence of phthisis, there was one point in support of this view. Lupus vulgaris, there was strong evidence to believe, was a tubercle of the skin. If that was the case, then the fact that lupus occurred about the same time as enlarged glands was in favour of Dr James's view. Then there was the occurrence of hair. Dr James had spoken of the occurrence of hair at the orifices of the body. There was one exception, the glans penis. On it there was no development of hair follicles at all. As to the connexion between the evolution of hair and physical strength, it had always occurred to him that this was to a certain extent a tradition, and that it had something to do with the occurrence of hair on Esau and Samson, who were proverbially strong men. He did not think it a sign of strength. He was inclined to consider it an evidence of diminution of vital energy, and in this he agreed with Dr James. There was one peculiarity about the manner in which hair disappeared. At the exterior portion of the eyebrows hair was apt to become less, while in the inner portion it was apt to become stronger as age increased. Then again on the chest, it was very curious to watch how the hair developed, how a few hairs grew first round the nipple, then extended outwards across the chest, further and further as age advanced. In women sometimes one met with this, and it was in support of Dr James's view. Very often where the mammæ were defective he had noticed the growth of hair round the nipple.

Dr Caird referred to tubercular disease of the joints and male genital organs. In a number of cases of tubercle of the testicle, they would find that the disease was most common from 18 to 20, just after puberty, even where venereal disease had not existed, to lay the foundation for tubercular mischief. The joints appeared to be affected in the same order which they followed in reaching full development. In the upper limb, the elbow was the

first to become affected by disease, then the wrist, and still later the shoulder-joint. The elbow was the first of these joints to have perfect ossific development. In the lower limb the hip was the first attacked, and then the knee and ankle.

Dr A. Smart said that but for the lateness of the hour he would have reviewed, not adversely, a number of points in the paper which struck him as of great interest. He would, however, restrict his remarks to a single illustration. Dr James had admirably pointed out that the vigour of tissue assimilation became progressively weaker from the period of adolescence until the maturity of adult life was attained, when the physiological balance was so nicely struck as to be easily disordered by any extra call for energy required in some given direction. Dr J. E. Pollock, in his "Elements of Diagnosis in Consumption," makes many interesting observations with reference to the conditions which favour the development of pulmonary phthisis. He would content himself by citing an example from these which he thought in point in corroborating Dr James's statement. Of 155 cases in patients who had recently been confined, analyzed by Pollock, in 48 consumption was developed during pregnancy, and in 51 it began at confinement. An analysis of 94 other cases in which the disease was developed after confinement, showed that it began during lactation in 54. In these instances the extra vital energy required for utero-gestation or for lactation still further weakens the nutritional activity of the pulmonary tissue, the tissue which first fails, and thereby precipitated the advent of the malady. He would only further add, that besides its other many recommendations, the paper has a special value in its recalling professional attention to the fundamental consideration that pathological tissue change is to be primarily found in departures from normal physiology. Dr James makes no allusion to the bacterial view. It is, I think, much better kept out of sight. The specific germ has, no doubt, an important, though only and always a *secondary* rôle to play in connexion with pulmonary consumption, but that part it can only play after the pulmonary tissue is impaired by a lowered vitality, brought about by defective tissue nutrition, or some other cause susceptible of being referred to some defect in normal physiology.

Dr M'Bride thought that the only point open to attack in Dr James's most original and suggestive paper was as to the subject of hair. Dr James seemed to look at the matter too much from the standpoint of evolution, assuming that all parts and persons were naturally hairy, and that this condition was only modified by external circumstances. He would like to ask Dr James how he accounted for the absence of hair from women's faces, the arms of dairymaids, and the axillæ of hairy breeds of dogs?

Dr James, in his reply, said that he had first of all to thank the Society for the way in which it had received his paper, and the

various members for their criticism. Owing to the lateness of the hour a detailed reply was out of the question, but there were some matters on which he thought he should say a few words. The first was on the growth of hair. He had to repeat what he stated in his paper, that in connexion with this the complicating circumstances were very numerous, but what he would venture to say was that if physicians in dispensary or hospital practice, or in insurance work, were to look upon excess of hair as evidence of weakness rather than of strength, they would probably meet with fewer contradictions. The period of occurrence of tubercle in the testes and larynx was another point of great importance, and bore out the theory proposed in his paper. As regards the question how deficient nutrition of a tissue led to the development of tubercle, he believed that the viewing of nutrition and reproduction as antagonistic processes would explain it. Given a tissue in which nutrition was impaired, reproduction would occur, and a less highly organized tissue would result He had discussed this in a previous paper in connexion with the etiology of tumours. As regards Dr Bramwell's remark, that at the age when pulmonary phthisis supervenes the organism is most exposed to unfavourable surrounding conditions, he was well aware that here again many circumstances had to be taken into consideration. He was concerned to-night, however, solely with growth and nutrition in connexion with phthisis, and, besides, the surrounding conditions referred to would not explain the development of brain or intestinal tubercle at earlier years.

Meeting XIV.—July 22, 1885.

Dr H. D. Littlejohn, *President, in the Chair.*

I. Exhibition of Patients.

Professor Chiene showed (*First*), a patient on whom he had performed LAPAROTOMY FOR INTESTINAL OBSTRUCTION. The patient was sent to the Royal Infirmary by Dr Donald of Leith on the 18th of March 1885, suffering from complete obstruction of the bowels of four days' standing. The symptoms at first were not aggravated, but on the 20th of March it was evident that an operation was necessary. The abdomen was opened by a median incision, and a band, the size of a No. 6 catheter, was discovered constricting a knuckle of the small intestine about eight inches from the ilio-cæcal valve. The constricting band was divided, and the intestines returned into the abdomen. The patient obtained complete relief, and after convalescence, which was delayed by

an attack of purulent bronchitis, he was dismissed on the 20th of April 1885. (*Second*), A patient who had been TREPHINED OVER NUMBER ONE AREA OF FERRIER, where he had received a blow seven weeks before operation. As a result of the blow he suffered from paresis of the right leg. The result of the operation was complete recovery of the motor power in the leg.

Dr James stated that, along with Drs Milne Murray and Graham Brown, he had had the opportunity of examining this patient before the operation. On the question of diagnosis they had all agreed, viz., that there existed a lesion in connexion with area one. They were also unanimous as regards the question of operative interference, although the history of injuries and some of the nervous symptoms which the case presented seemed to indicate that there might be other lesions than the cortical one, of less importance, certainly, but still which might be sufficiently grave to render recovery after the operation less complete. He was, however, extremely pleased to find the result so satisfactory.

Mr Joseph Bell alluded to the mydriasis which follows injuries of the forehead, and is sometimes permanent, and also suggested the possibility that the obscure relations of symptoms to seat of injury might be explained by contre-coup.

II. EXHIBITION OF SPECIMENS.

1. *Professor Chiene* showed the preparations in the Surgical Museum in the University of Edinburgh illustrating FOREIGN BODIES IN THE PHARYNX, ŒSOPHAGUS, AND AIR PASSAGES. He directed attention to the importance in all cases of sudden death of carefully examining the pharynx; he showed in this connexion a piece of meat which had been found in the pharynx of a subject in the dissecting rooms, in whom heart disease had been certified as the cause of death. He pointed out the importance in all cases of sudden choking of passing the finger into the pharynx in order to displace any foreign substance which may be obstructing the breathing. He pointed out the value of bulbous œsophageal bougies, and described a case, showing the specimen, in which he had, with the use of the cylindrical bougie, failed to discover a bone in the œsophagus, which had afterwards caused the death of the patient by ulceration. The bulbous bougies were also of value in the diagnosis of the exact situation and dimensions of a stricture of the œsophagus. He believed that as the value of bulbous bougies was generally recognised in making an exact diagnosis in stricture of the urethra, and had displaced cylindrical bougies, so also in stricture of the œsophagus and rectum, bulbous bougies would displace cylindrical bougies now commonly used in these canals. In showing the specimens illustrating foreign bodies in the air passages, Mr Chiene mentioned that the late Mr Spence believed that foreign bodies most commonly found their way into the left

bronchus. This fact was illustrated by several specimens shown. The majority of surgeons believed that foreign bodies generally find their way into the right bronchus; his own experience was in favour of the right bronchus. He showed a patient who came to him some time ago, stating that he had swallowed a half-sovereign; he stated that he felt the foreign body on the right side of his chest. Auscultation indicated decreased respiration on the right side. In his case tracheotomy was performed, and the patient inverted, but no result followed, the wound was allowed to heal, and the patient left the hospital. He still states that he feels the foreign body, and he states that it sometimes moves. On the suggestion of the President, Drs Affleck, Smart, James, and Russel Wood examined the patient's chest; the result was negative. In connexion with such cases, whenever the history or the examination of the chest render probable the presence of a foreign body, Mr Chiene believed it was safer for the patient to perform tracheotomy before inversion.

2. *Professor Chiene* showed a CATALOGUE OF THE SURGICAL MUSEUM IN THE UNIVERSITY. It had been carefully drawn up last year by his assistants, Mr George Mackay, M.B., C.M., and Mr Thomas Fraser, M.B., C.M. The museum had been mainly formed by the late Professor Spence, and the completed catalogue now shown would enable any one working at any special branch of surgical pathology at once to find specimens bearing on the subject. While the catalogue was drawn up mainly for the purpose of facilitating the teaching of surgery, he need not say that it was always open to any one desiring information on any special subject.

Mr Symington stated that, as a result of a careful examination of frozen sections of children, he had not been able to satisfy himself of any appreciable difference in the direction of the two bronchi. Any difference that might exist would become more obvious in the adult, but, in any case, he believed it to be very slight.

Dr Shand regretted he had not been able to lay his hands on the specimens of the windpipe cases, though he had them in possession. He said this subject had early engaged his attention, as several very interesting cases had presented themselves while he was resident in the Royal Infirmary, and some time before he graduated he had exhibited at the Royal Medical Society a large piece of a nutshell, which, after being lodged in the air-passages for some weeks, was ejected in a violent fit of coughing the morning of the day when it had been arranged that he was to perform tracheotomy, an operation which he had several times been called to perform when acting as a house-surgeon in the hospital. No irreparable mischief had been sustained by the lungs, and the patient got quite strong soon after. The shell in this case was not stationary, but oftener in the trachea than elsewhere. The next case he had to notice was of a foreign body of some gravity, and

well fitted for a Brunell mode of treatment, the foreign body being a piece of lead. It had passed from the mouth into the windpipe of a boy of about eight years of age. The father, with the boy at his side, addressed him (Dr Shand) as he was hurrying off on a professional round many years ago now. The statement was,—the boy had swallowed a bit of lead, and suffered from cough and breathlessness. A hasty application of the stethoscope confirmed this statement, as a slight whistling sound was perceptible over the right bronchus. In short, it appeared to have gone, as Scotch boys say, "down the wrong throat." As he had long before resolved on treating a case of this nature *à la* Brunell, and observing that the father had an expression of intelligence, he at once decided on a copartnery,—himself as the medico, and the father as the chirurgical. He (Dr Shand) gave him the necessary orders to carry out on his return home. A week or so afterwards the porter's lodge he lived in, a few miles from headquarters, was opened to him by a woman, who, on recognising the doctor, exclaimed, "We have got the lead the boy swallowed," and rushed into the cottage to get it. It was the size and much the shape of a printer's letter type. On inquiry, she said her husband, after returning home, didn't put the boy over the side of a bed, but suspended him over the stone wall (over 4 feet high), gave him "a wheen hits atween the shouthers," and that, after a cough or two, the lead dropped into her apron, which she held out for it, and that the boy had felt no uneasiness since. In regard to foreign bodies in the œsophagus, Prof. Chiene's case of the bone, which was not capable of detection by the ordinary probangs and bougies, had some points of similarity, as the late Prof. Spence's case of teeth and plate had other points of similarity to a case that occurred in Edinburgh, and which, he believed, they would find reported in the *Edinburgh Medical Journal* of 1842 or 1843, by Dr J. Duncan. The patient was a dentist's apprentice, who had equipped himself with a set of false teeth set in a metal plate. One night he left them in his mouth, and next morning, recognising his loss, he came to the Infirmary to see if they could be discovered in his gullet, or elsewhere. There was sufficient uneasiness in swallowing to indicate the probable presence of a foreign body, but notwithstanding the careful and skilful examinations of each and all of the staff of surgeons of the institution, with probangs, probes, and forceps, nothing was detected, and the probangs were passed with ease over the whole passage of the œsophagus. After a few days at his home in Dundas Street, the young man suddenly expired in an attack of hæmoptysis. The late Dr James Duncan was summoned at once, and, accompanied by Dr Shand (who was then Dr Duncan's house-surgeon), he lost no time in reaching the case, but all was over. The autopsy explained the peculiar features of the case very simply. The metal plate was lodged transversely, so as to present the vacant arch as a passage for the probangs, and a

fold of the soft textures of the œsophagus was lapped over the upper edge of the plate so as to conceal the metal almost from view, and the hæmorrhage was found to arise from the cornu or sharp corner of the metal plate having cut its way through into the arch of the aorta. With permission of the President, Dr Shand begged to add another remark, somewhat in contrast to the usual grave character of their discussions. This was, that in Prof. Spence's case of false teeth and its sad termination he read the explanation of an amusing occurrence in a case some two or three years before Mr Spence's death, where he (Dr Shand) was the administrator of the chloroform, and Mr Spence the operator. Just as the patient was about ready for operation, Mr Spence, with an expression of alarm, hastily slipped up to the head of the patient, and muttering something about "very dangerous," attempted to extract the beautiful regular white teeth of the lady with his fingers, but in vain, for though the patient was a sexagenarian, the teeth were her own, and indigenous to the soil. It was a repetition of the English lady who was married to a Yankee, and was advised by her sister-in-law to take her teeth out at night. She protested about it to her husband next day, who responded,—"But, my dear, it *is* dangerous, you must do it to-night in any case."

3. *Dr Allan Jamieson* exhibited some specimens of MEDICINAL SOAPS prepared by Mielck, of Hamburg, at the suggestion of Dr Unna.

4. *Mr Cathcart* showed SPECIMENS and described a MATERIAL FOR MAKING CASTS. He said that about four or five years ago his attention was directed to those elastic faces which are sold in indiarubber shops as being made of a material which might be very useful for casts of pathological conditions, such as ulcers or tumours. On inquiry, he could gain no information as to their composition, so he had to set to work to find it out for himself. After a good deal of experiment he found that glue and glycerine would make a basic substance for the casts in a way that more than answered his expectations. The method of preparing the substance was published in the *Clinical and Pathological Journal* for 22nd March 1884, but as some might not have noticed it, he had thought it worth while to bring it under the notice of members of the Edinburgh Medico-Chirurgical Society, as an extended trial has only confirmed its usefulness. Before stating its composition, he said that Mr Bailby, then curator of St Bartholomew's Hospital Museum, had published a somewhat more complicated recipe for an elastic material for casts, which would be found in the *British Medical Journal* for October 1882. The substance Mr Cathcart used was made as follows:—Of strong clear glue or gelatine (not necessarily prepared) take by weight, say 4 oz.; of glycerine (pure is best, but the coarse kind will do) take by measure 4 oz.; of water,

of a globular form. The skin over it is stretched to a certain extent, and the scrotal veins are prominent. The cord is normal.

In the belief that the growth was probably sarcomatous in character, it was extirpated on the 3rd October, and the patient made a rapid recovery. The tumour was injected with gelatine and carmine through the vessels of the cord, and laid aside to harden in alcohol. On bisection a small quantity of serous fluid escaped.

On examination it was found to measure about 3½ inches in length, 2 inches from side to side, and 2½ from behind forwards. In shape it was somewhat oval, with an obscurely bossed surface. It felt firm and elastic. The tunica vaginalis was normal. The epididymis and cord were spread out along its posterior surface, and it was capped by the stretched and flattened out remains of the testicle. The cut surfaces displayed a mass of whitish rounded bodies embedded in the midst of a dense injected matrix. There was a large irregular cavity lying towards the posterior aspect of the tumour. Interstitial septa could be traced running towards the periphery of the tumour where they blended with its coverings. Figure 1 represents the appearance presented.

On closer examination the rounded bodies were found to have a zoned character where seen in section, as if they were made up of various concentric layers. They could readily be picked out entire (see Plate), just as one might turn out a wen, and there was left a cavity with smooth whitish walls. It frequently happened that some of these pearl-like bodies had several centres, as if three or four cysts had fused, and in such cases the cavity had alveoli or depressions corresponding to the number of centres. In no case did we find a communicating chain of cysts. The large irregular cavity appeared to show that a series of cysts had fused and broken down; its walls, moreover, were not vascular, in this respect resembling the septa, with which they were continuous, and pointed to some failure of nutrition. The walls of the cavity were lined with a thick white cartilaginous-looking layer, covered in its turn with a granular pultaceous mass. This under the microscope was found to consist of breaking down epithelial scales, fat globules, cholesterine crystals, and granular *débris*.

Sections under the microscope brought interesting details into view.

The general groundwork of the tumour was seen to consist of a firm vascular connective tissue, showing here and there deposits of leucocytes. Small nodules of hyaline cartilage were also present. The glandular part of the growth consisting of cysts might be divided, according to the character of the epithelial lining, into three sets:—

1*st*. The greater number of the cysts were lined with tesselated epithelium. They possessed a true Malpighian layer with prickle cells, and over this a horny superficial layer, the squames of which gradually coalesced into an amorphous mass. Fig. 3.

2*nd*. There were a very few lined by a cylindrical epithelium.

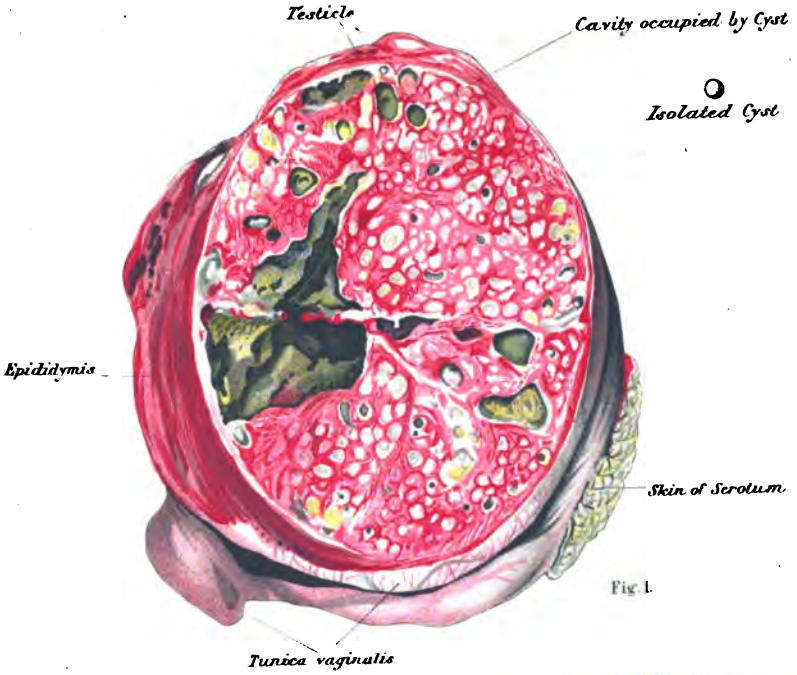


Fig. 1.

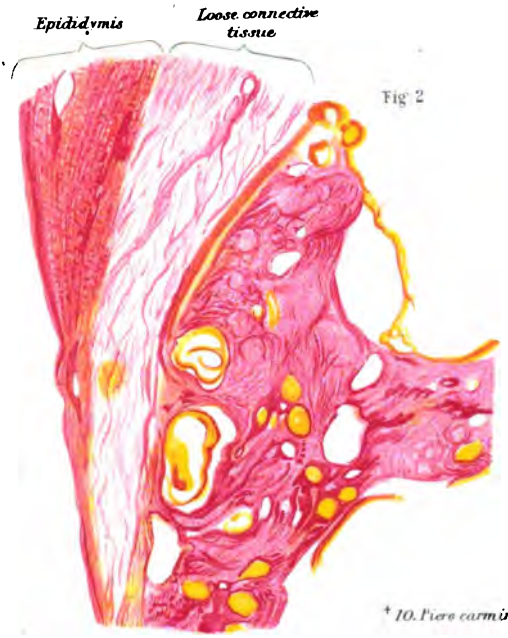


Fig. 2

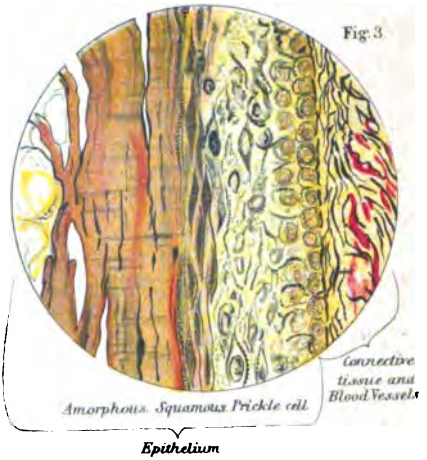


Fig. 3.

+ 350. Picro carmine

CYSTIC DISEASE OF TESTICLE

F M C ad. nat. del.

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3rd. Here and there, as shown by the smaller yellow masses in Fig. 2, there were cyst-like cavities which contained a closely-packed granular amorphous plug, apparently epithelial.

The tumour was enclosed in a dense capsule of tunica albuginea. The testis lay quite outside this, and had a fibrous envelope prolonged over it also, but was distinctly separate from the tumour. The epididymis showed fatty degeneration of its epithelium.

Beyond the presence of cylindrical epithelium, common also to the tubules of the rete testis, we have little to guide us here in regard to the development or true site of this new growth. The epididymis and testis seem, however, to be perfectly distinct from it, although the histology so far favours the view taken by Curling that such cystic disease originates in the rete testis.

A further examination of this and other specimens, kindly provided by Mr Chiene, may, however, shed light on the matter at some future period.

EXPLANATION OF PLATE.

FIG. 1.—Section of tumour, natural size.

FIG. 2.—Portion from periphery of tumour, under a low power, showing the manner in which the epithelial lining peels off from the cyst wall.

FIG. 3.—Wall of cyst and epithelial lining.

The President asked how it was that the literature on this subject was so great while cystic sarcomata were not so common?

Professor Annandale said he thought that there was a third specimen of this tumour in Edinburgh. He had one somewhat larger, but very like Mr Caird's specimen, which had been removed by Mr Syme. He would be glad to have it examined by Mr Caird.

Dr Caird said he thought the literature on this subject was large, from the fact that when a specimen occurred it was in all probability immediately reported. The tumour had grown slowly, and had not greatly inconvenienced the patient.

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